Essays on the Workforce of the Developing World

by

Benjamin Thompson

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in the University of Michigan
2017

Doctoral Committee:

Professor John Bound, Chair
Assistant Professor Achyuta R. Adhvaryu
Professor Charles Brown
Professor David Lam

Benjamin Thompson

bpthomp@umich.edu

ORCID iD: 0000-0002-0570-1709

For my family.

Acknowledgments

I'd like to thank my advisors, Achyuta Adhvaryu, Charlie Brown, David Lam, and especially my chair, John Bound, for their patience and sound advice. I am also grateful to Gaurav Khanna for new ideas, to Mike Gelman for great debates, to Fudong Zhang for international friendship, and to many others I've met along the way.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ABSTRACT

Essays on the Workforce of the Developing World

by

Benjamin Thompson

Chair: John Bound

My dissertation focuses on the outcomes and consequences of public policy in a developing country, specifically, that of Brazil. In many ways, Brazil represents a microcosm of the emerging world: the nation has a large, fast-growing economy, geographic diversity, and a diverse population, both racially and socio-economically. I maintain that much can be learned about growing and developing economies from studying Brazil. Each chapter of my dissertation analyzes an important aspect of labor markets in Brazil. The first chapter entitled "Pensions, Retirement, and the Disutility of Labor: Bunching in Brazil," focuses on how Brazilian elderly workers react to pension eligibility as part of a long-running and generous pension program that guarantees a high fraction of worker wages to be paid out upon claiming. Interestingly, while a worker is not required to retire to receive the pension, about 45% of urban workers do anyway. I explore the potential reasons for this phenomenon from an economic modeling standpoint and find that not only does the inability to borrow matter, but the excessive generosity of the pension does also. My results have implications for the formation of retirement incentives in the developing world and offer insight into how aspects of such policies may create predictable variation in employment patterns. The second chapter focuses on the effects of government spending in Brazil: namely, what happens to employment when local governments come into control of a larger amount of resources (in our case, federal transfers). Using a means of identification that relies on the assignment of federal funds to municipalities dependent on arbitrary population thresholds as criteria, we find that employment does increase by a larger factor than has been documented in

developed countries such as the United States. We explore potential explanations for this finding, noting that the effect is concentrated among low-skilled and private sector workers. This chapter is co-authored with Breno Braga and Digo Guillen. The third and final chapter of my dissertation, “Extending the Social Safety Net: Female Labor Supply and Pension Eligibility,” examines another arm of the social security system that grants old-age pensions to rural workers. Because of a policy change in the early 1990s, large numbers of rural female workers were suddenly made eligible for a sizable pension conditional on working requirements, whereas they had not been eligible at all before. I find that female workers respond by increasing their labor supply to qualify for the pension, and importantly, I also find that younger rural females who will be eligible in coming decades also increase their labor supply compared to their urban counterparts. My results from this study help in understanding the flexibility of the labor supply decisions of older workers and shed light on the responsiveness of younger generations to future incentives. The issues I tackle in the Brazilian context are increasingly growing concerns among policy makers: specifically, that of how to stimulate an economy and how to plan social assistance programs in the context of an aging workforce. As other developing countries, not only in Latin America but elsewhere, plan public policy agendas, I hope that my research can be informative.

CHAPTER 1

Pensions, Retirement, and the Disutility of Labor: Bunching in Brazil

Elderly workers in developing countries face certain issues, such as credit constraints, in their retirement decisions that may not be as common among their counterparts in the developed world, and these concerns may lead workers to work more or less than their preferred number of years. In this study, I firstly use regression discontinuity methods to show that a large fraction of urban male heads of households in Brazil (roughly 45%) react contemporaneously to pension eligibility by retiring. Because retirement is not required to receive the pension and because the return to working does not change discontinuously at the eligibility cutoff, workers should not react contemporaneously unless additional constraints (such as credit constraints), are at work. Secondly, I develop a model of retirement decisions that explores how pensions in the face of credit constraints can influence such decisions and how bunching can be expected in an environment with borrowing constraints and a generous pension. I discuss applications of this model to determine how the observed behavior in conjunction with the model can be used to make inferences about welfare and labor supply decisions in the face of different pension values.

1.1 Introduction

Countries around the world are in the midst of, or just emerging from, social security reform as they face aging populations. In East Asia, China recently has dropped its famous one-child policy, in part to hopefully increase the ratio of working to retired individuals in order to help pay for its social security system, and South Korea has increased its minimum retirement age (SSA (2016)). In Latin America, Ecuador has recently passed legislation to cut direct government costs of social security, and in Chile, a recent report on the state of social security has shown that while progress has been made due to reforms within the last decade, more progress is still necessary for a more stable system (SSA (2015a), SSA (2015b)). In part, these reforms have been suggested in order to address the solvency of various retirement systems which are feared to be facing a steep elderly burden in coming years; however reforms have also pointed to a need to increase coverage and reduce old-age poverty.

Retirement incentives in the developing world demand examination for a number of reasons, and among these, I identify two that are of particular importance. First, compared to the number of studies done on developed countries, the number of studies examining financial incentives for retirement in emerging economies are relatively rare. In particular, issues prevalent in developing countries such as borrowing constraints and weak institutions may complicate the interaction between retirement benefits, such as pensions, and labor supply. Second, as many countries are facing situations in which they would like to keep elderly workers in the labor market, it is important to obtain some kind of idea of how much an additional year of working is worth to elderly workers. Such an estimate could be used to evaluate the welfare costs of policies such as a minimum retirement age.

In this paper, I examine labor supply reactions to retirement eligibility and the welfare cost of labor in the context of Brazil. Brazil is home to one of the world's largest and most generous social security systems, with around 28 million beneficiaries in 2010. The size and generosity of the social security program has raised concerns as disbursements in 2010 reached US$153 million, accounting for 6.9% of GDP (da Silva (2012)). Calls for reform include reductions in benefits and a minimum retirement age, however little is known about the welfare effects of the institution of these policy changes.

I find firstly that workers react contemporaneously to anticipated changes in income; upon receiving an old-age pension at the age 65, urban male household heads are over 40% more likely to leave the labor force. Secondly, I develop a dynamic model of optimal retirement and consumption. This model indicates that the response I find is generated not by borrowing constraints alone, but also by the generosity of the pension: in short, borrowing constraints are a necessary, but not a sufficient condition to generate the observed reaction. Finally, I explore brief applications of my model first to the calculation of welfare and second to the estimation of the distribution of labor supply among elderly workers.

The majority of the existing literature has concluded that financial incentives themselves do matter in determining retirement (Gustman and Steinmeier (1994); Rust and Phelan (1997); Stock and Wise (1990)). Belloni (2008) notes that a new generation of retirement studies have relaxed former assumptions and have utilized more up-to-date statistical techniques to estimate retirement effects. For instance, French (2005) relaxes the assumptions of certainty and perfect credit markets. While the effects of pensions on incentives and in turn on retirement behavior have been well-documented, the overwhelming majority of these studies focus on developed economies.

The evidence that financial incentives in the *developing* world matter is noticeably less copious, however it seems to indicate that they do. Studies examining retirement incentives around the world are far less common than those examining retirement incentives in the United States and Western Europe; there is no corresponding collection of studies rivaling Gruber and Wise (1999) focusing on the developing world. However, there are a few relatively isolated studies that examine incentives for labor force withdrawal among elderly workers. Ranchhod (2006) examines the famous South African Old Age Pension, finding labor force withdrawal among eligible workers; however, the author is unable to determine if the withdrawal is due to a pure "income" effect.[1] de Carvalho Filho (2008) studies the extension of pension benefits to rural workers in Brazil, finding sizable, negative estimates on labor supply: access to old age pension benefits are estimated to reduce the chance of not working by roughly 38 percentage points and total hours worked by over 22 hours per week. Legrand (1995) also studies the Brazilian pension system, finding results consistent with the notion that the system did play a role in retirement decisions using data from the 1980 Brazilian census, finding that those with contribution history to a pension system had higher retirement rates, and also finding spikes in retirement around the ages of eligibility. Though this latter result is similar to what I find here in this paper, the system under consideration in Legrand (1995) was noticeably different with one requirement for benefits being stopping employment.

In spite of the fact that de Carvalho Filho (2008) examines part of the same program I study, I maintain that the studies most related to mine are Kahn (1988) and Ranchhod (2006). The distinction is that these two studies examine the effects of *anticipated* pension benefits on labor supply, whereas de Carvalho Filho (2008) studies a legal change that suddenly made a large fraction of rural workers eligible for benefits. In the context of dynamic optimizing behavior, expected future financial incentives to retire can and usually do have different effects than those that are unexpected.

My paper contributes to the literature on pensions and social support in developing countries in three main ways. Firstly, while my study focuses on an expected pension, and in this way examines the "long run" effect of pensions on labor supply, as do Ranchhod (2006) and Kahn (1988), I am able to identify a response within only a months of eligibility; prior studies find that individuals tend to retire at some point within the age of eligibility, lending credence to the possibility that social norms are at work; a contemporaneous response at the monthly level would seem to indicate otherwise. Secondly, I seek to quantify the welfare effects credit constraints and pension generosity have on workers with the use of a model. To my knowledge, no other paper examines pension generosity in conjunction with credit constraints to determine how changes in either credit constraints or pension generosity can

[1] In this paper, I use the term "income effect" somewhat loosely to refer to changes in economic behavior induced by an increase in income that is either anticipated or unanticipated. While it should not typically be expected that anticipated income changes have a contemporaneous effect, this is exactly the type of behavior I study in this paper.

change societal welfare. My results can thus inform policy makers concerned with incentivizing (or dis-incentivizing) labor force participation among the elderly. As such, I describe some brief applications taking into account the implications of my model.

1.2 The Brazilian Old Age Pension Program

The central source of variation in anticipated income I explore in this paper results from a publicly funded old age pension policy that has been in place for several decades. The program relies on an age cutoff, which provides the plausibly exogenous variation in anticipated income given that I have data on each respondent's birthday (and hence, his date of eligibility). I use this variation to estimate the labor supply response to the reception of this anticipated income.

I begin this section by describing the policy that I am studying and its associated rules. In Brazil, there are currently two major routes to receiving a public pension, and so strictly speaking, the policy I study is actually one of two eligibility arms. The first is via length of contribution to the social security system, under the *aposentadoria por tempo de contribuiçao* ("retirement by length of contribution"). This first arm stipulates that upon contributing to the social security system (usually by holding a formal sector job) for 35/30 years for males/females, workers are eligible to receive a function based on their past earnings, which I will describe below. The other route, which is the focus of this paper, is via age, under the *aposentadoria por idade* ("retirement by age") program, which stipulates that upon turning 65, males who have contributed to the public social security system for 15 years and who live in urban areas are eligible to receive a pension. Females who satisfy the contribution requirement of 15 years and live in urban areas are eligible to receive the pension at age 60. Rural male and female workers are eligible to receive the pension five years before the urban counterparts, at age 60/55 for males/females. Importantly, retirement is not required to receive these benefits. Individuals can continue to work and receive the benefits. [2]

1.2.1 Incentives to Retire

These are defined benefit pensions; however, the calculation of benefits under the *aposentadoria por idade* is not unfortunately straightforward and requires some careful explanation. There are two main

[2] As outlined in Legrand (1995), the Brazilian social security has a long history dating back to several decades before the time period I study. I do not seek to perform an analysis of retirement behavior before and after the program I study was implemented (and it is unclear if one is even really possible), but rather I seek to examine the "effects" of one aspect of the system.

time periods during which pension benefits were calculated in different ways. In many ways, the differences in the calculation of benefits over these periods revolve around an important pension reform during 1998-1999 that mostly affected the "time of contribution" pension, however did have some implications for the "retirement by age" program that I study. These pensions are subject to income taxation in that they are considered income; however, they are not subject to additional taxation by virtue of being benefits. Soares (2010) provides a thorough explanation of the system, I referred to his paper in the writing of this section.

1.2.1.1 Pre Reform (Pre-1999)

During the first period, before 1999, pension benefits (B) were calculated using a relatively straightforward formula. Pension benefits were paid out according to the formula

$$B = 0.7(BW_{pre1999}) + 0.01(BW)(j) \tag{1.1}$$

Where B represents the pension benefit, $BW_{pre1999}$ represents the 'benefit wage', or the average of the 36 monthly earnings just before application for the pension, and j represents the number of years of contribution. The total benefit payout was capped at 100% of the benefit wage, so that men/women reaching age 65/60 with at least 30/25 years of contributions would have an incentive to delay retirement only so far as their wages were increasing and thus increasing the average toward the end of their working life. For those reaching age 65/60 with less than 30/25 years of contributions, benefits would continue to accrue with additional years of contributions. Because 15 years of contribution were required at age 65/60, the benefit pays out a minimum of 85% of the benefit wage. Additional years of contribution were included on top of this fraction – for instance, an urban male worker retiring at age 65 (as soon as he was eligible) who had a contribution history of 19 years would have received 89% of the benefit wage. Note further that while retirement was not required, if an individual works additional years after claiming the benefit, the benefit would not continue to increase with additional years of contribution. That is to say, every year an individual waits to claim the benefit while still remaining in the labor force and contributing, the benefit upon claiming increases by at least 1%, and benefits stop accruing upon retirement.

1.2.1.2 Post Reform (1999-Present))

Since 1999, there have actually been two formulas used to calculate potential pension benefits. For the most part, the first formula is used; however, the second formula can be used under certain conditions. The first formula is

$$B = 0.7(BW_{post1999}) + 0.01(BW)(j) \tag{1.2}$$

Where B represents the pension benefit, BW again represents the 'benefit wage' and j represents the number of years of contribution. This new benefit wage is calculated using the average of the top 80% of monthly contributions. The benefit paid out according to this formula is 70% of the benefit wage with an additional 1% added for every year of contribution up to a maximum of 100% of the benefit wage, which is similar to the pre-1999 formula. Again, this formula yields a minimum of 85% of the benefit wage for anyone meeting the criteria for the old age pension.

The second formula is

$$B = (BW)(FP) \tag{1.3}$$

Where B represents the pension benefit, BW represents the 'benefit wage' and FP represents an adjustment factor called the *fator previdenciario*. As in formula (1), the benefit wage is calculated using the average of the top 80% of monthly contributions. The *fator previdenciario* serves to adjust the benefit wage for demographic trends, age, and length of contribution. Upon eligibility for the old age pension, this second formula is used only if the *fator previdenciario* is greater than one. Generally speaking, this amounts to the *fator* being used "advantageously," i.e. only if it results in a larger calculated benefit than the first formula. The *fator* (FP) is released in table form every year based on population estimates of the previous year. It is calculated according to the formula

$$FP = \left(\frac{(tc)(tax)}{L}\right)\left(1 + \frac{age + tc + tax}{100}\right) \tag{1.4}$$

where tc indicates time of contribution, tax is the social security tax on labor income set at a constant 31%, age is the age of the beneficiary upon retirement, and L is the remaining life expectancy of the beneficiary, conditional on age. The following table shows the benefits one can expect to receive as a fraction of the benefit wage upon retiring at a given age and contribution period if one were to consider retirement in 2013, the last year of my data. Values over one indicate the use of the *fator* in the calculation of benefits.

When the *fator* was first introduced, the law allowed for a 5 year (60 month) transitional period; thus for the years 2000-2004, a weighted average of the *fator* was used, given by the following formula:

$$F_t = (FP)\left(\frac{t}{60}\right) + \left(\frac{60 - t}{60}\right) \tag{1.5}$$

where t represents the number of months that had passed since November 1999 at the time of application.

Under both ways of calculating pension benefits, the benefit wage BW is bounded above and below, meaning there's an effective upper bound on the pension benefit – Workers who have earnings profiles that are below this bound will experience high replacement rates, whereas workers who have earnings profiles that are above (or lie at least partially above) may not have such high replacement rates. The benefit ceiling changes from year to year and is a multiple of the benefit floor, which is held at the annually-adjusted value of the national minimum wage. The ceiling is usually between 5 and 7 times the value of the minimum wage – in 2003, it was roughly 7.8, and in 2012 roughly 6.1 times the value. [3]

Note also that even though benefits are given in nominal terms, they are indexed to account for inflation using a national price index. Inflation has been a concern for Brazil in the past and the government has made efforts to tie all benefits (not simply the retirement benefits) to an index of some sort, which in many cases happens to be the minimum wage.

1.2.1.3 Are Binding Credit Constraints Necessary to Explain the Behavior?

The pension stops accruing upon claiming, and does not increase in value after claimed. Given that it is in all conceivable cases rational to claim the pension as soon as one becomes eligible (as the accrual rates from working an additional year are far too low to compensate a worker with an average conditional life expectancy for a year's worth of eligible pension), this changes the rate of total compensation around the point of eligibility. [4] In other words, there is an effective "kink" in the individual's budget constraint upon reaching the age of eligibility, assuming that he or she claims the pension as soon as is possible. Another way to see the presence of the kink is to note that any years worked, up to 55 years of contribution, increase the pension received when the recipient becomes eligible. Once the pension is claimed, any additional years do not increase the pension received or the overall present discounted value of pension wealth.

[3]While my results do not depend on knowing the specific pension amounts, a large part of my sample reports total income under this ceiling – roughly 75% of urban men under the age of 65 *not* receiving a government pension reported total income less than 4.6 times the minimum wage. I conclude that a sizable portion of the population I study faces high replacement rates.

[4]To see that claiming the pension as soon as possible is rational, consider that the choice of whether to claim at 65 or a later age (66) can be stated as between the following alternatives: (1) Claim when eligible (age 65), receive pension d_1, keep working and earn wage y or (2) Claim one year after eligible (age 66), keep working and earn wage y, receive pension $d_2 > d_1$ in the following year, but forego one year of d_1.. For Choice 1 to dominate Choice 2, foregone pension d_1 should dominate the a fraction of the increased pension wealth, where the fraction is $\frac{LE_{age=65}-1}{LE_{age=65}}$. Given that life expectancy (LE) at age 65 in Brazil was been roughly 81 over 2001-2013 according to the OECD (`https://data.oecd.org`), and d_2 is roughly about 1.05*d_1 at 65, Choice 1 dominating Choice 2 corresponds to almost any positive discount rate.

However, the size of this kink is relatively small, and I will argue too small to generate the behavior we see. The size of the kink can be calculated by examining how much pension accrual changes with additional years of labor. For instance In the last year of our data, 2013, claiming retirement with an additional year of work history at age 65 results in a pension that is, on average, 2.3% higher; therefore, the kink in the 2013 budget constraint due to pension accrual is roughly in this neighborhood.[5] Other years follow similarly, as pension accrual is relatively constant across years. [6] In a standard neoclassical model of labor supply (such as that outlined in Macurdy (1981) for instance), the presence of such a kink in the budget constraint could imply that workers, if they adjust on a margin of labor supply, are not reacting solely to the increase in income, but are also substituting toward the cheaper good of leisure. Now, if we assume that the response was *solely* due to the substitution effect and credit constraints are *not* at work, this would imply, given the retirement rate of roughly 46%, a participation of elasticity of 20, which is by any measure implausibly large. [7] I thus maintain that even in the face of the slight "kink" in the budget constraint at the age of eligibility, credit constraints are the most plausible candidate explanation.

To think of things in slightly different terms, suppose binding credit constraints are not present, and agents can smooth pension income over the course of their lifetime, borrowing against the pension benefit. If workers pick the age at which they will retire first and then make the decision of when to claim accordingly, it would be highly curious that all workers decided to pick an optimal retirement age that was the same down to the monthly level. Estimates of behavioral responses, even if they are on the margin of the amount of labor supplied, do not represent estimates of labor supply but rather of the "consumption" value of labor – how much employment would shift if individuals experienced a change in their non-labor income.

[5]To understand why the change in the pension accrual rate is the kink, consider the following scenario: a 64 year old worker faces the decision of working an additional year when he turns 65. Prior to turning 65, his total compensation is his wage and the increase in the present discounted value of the pension he will receive. After turning 65, he has claimed the pension, which will not increase, hence his return to work is then only his wage. One may wonder about a worker who is turning 65 with 14 years of experience and who will become eligible for the pension upon his 15th contribution year; however, this situation is no different as his return to working prior to 65 is his wage and the change in the pension amount, and his return to working after 65 is again the wage. The value of the present-discounted value of the pension for this worker is increasing up until 65, at which it stops increasing. It is important to note here the because the pension, once claimed, is independent of work, the difference in the return to work before and after age 65 is simply the change in the size of the eventual pension.

[6]This can be verified from the *fator previdenciario* tables publicly available from the INSS. These tables are online at `http://www.previdencia.gov.br`.

[7]A review in McClelland and Mok (2012) finds estimates in the literature to be substantially lower than this number. The high-end estimates of participation elasticities tend to be slightly over 1. The hours elasticities seem to be between roughly 0.1-0.3 for prime-age males. Assuming that elderly workers are twice as responsive at an hours elasticity of 0.6, the change in the pension accrual rate should then lead to a drop of roughly 5 hours worked per week – a figure that falls far short of the labor supply reductions shown.

1.3 Data

My data comes from a widely-known, publicly available annual household labor force survey called the Pesquisa Nacional por Amostra de Domicílios (PNAD). Analogous in some ways, though not entirely, to the U.S. Current Population Survey, the survey provides a snapshot into the economic conditions of over 100,000 households on a yearly basis. Importantly, the survey provides a great deal of demographic information (most important of which is the birthday of the respondent) and household conditions such as income. I use the available survey years from 2001-2013, omitting one year of survey data (2010) during which the PNAD was not carried out. I focus on urban male heads of households in my analysis; I therefore only use variation around the cutoff of age 65 (for males).

I make several important restrictions to the PNAD samples for my analysis. Firstly, I exclude households with a per-capita income of less than 25% of the annual minimum wage, due to an overlap with a social assistance program intended to help the extreme poor which I will discuss in more detail in the section below. Secondly, for most of my analysis, I consider observations within 5 years of the date of pension eligibility. Thirdly, and finally, I exclude observations within 1 month post-eligibility, to allow for households to being receiving funds. My final sample includes 76,255 household heads from 16 cross-sections.

Table 1.1 shows a summary of the main sample I use for my analysis. Most of the household heads under consideration do not live alone – the average number of household members is 3.25. As I focus on geographically urban areas which include most major cities, my sample is not particularly low-income, either – the average head's monthly household income is a little more than R$1,400 (roughly $1300 in 2000). Slightly more than half of the heads in my sample are in the labor force or working, while their spouses participate in the labor market to a much lower extent. The majority of heads also have a secondary education or less. Unfortunately, not all respondents who answered the three main questions of interest (receipt of government pension, employment activities, and labor force participation) answered other questions pertaining to household status. Non-response rates for household income were relatively low; however education non-response rates were relatively high.

1.4 Estimation and Results of Labor Supply Response

1.4.1 Validity of the Discontinuity

In order for a discontinuity to be usable as an identifier of a causal effect, certain assumptions must be made. These assumptions usually include (1) no manipulation of the running variable and (2) exogeneity of the cutoff value, and in fuzzy designs. This section addresses whether these assumptions are reasonable in the context of the policy I study.

The first assumption of no manipulation of the running variable in the context of the pension discontinuity is more than reasonable. Since actual age is not manipulable, the main concern would be with reported age; however, because the agency that conducts the annual PNAD (IBGE) does not keep track of or distribute pension funds, there is virtually no incentive to report different age values other than one's true value. The second assumption of the exogeneity of the cutoff is more concerning, since age 65 is an age at which we would expect some overlapping retirement policies in place. Indeed, there is another program that has an age cutoff of 65 – the *Benefício de Prestação Continuada* (BPA) program provides non-contributionary social assistance to the disabled and those over age 65 in households with a per-capita income of less than 25% of the minimum wage. The program provides a monthly payment of one monthly minimum wage to eligible individuals, and the eligibility of participants is periodically reviewed (Queiroz and Figoli (2010)). To address the concern that benefits paid out from this program might confound my results, I restrict my sample to those with monthly household income above 25% of the minimum wage in the given year. Note that this fraction of observations (as a fraction of the entire relevant sample) is a little over 5%, so accounting for the program does not substantially reduce the sample size. Aside from the BPA program, to my knowledge there are no other social assistance programs (or government programs in general) that use the age of 65 as a cutoff. [8]

1.4.2 Estimation Strategy

My estimation of the effect of the pension on labor supply variables is based on a regression discontinuity design (RDD) in the vein of Hahn et al. (2001) that makes identification and estimation straightforward. As aforementioned, conditional on contribution time, all male workers in Brazil are eligible for the *aposentadoria por idade* pension benefit. Using this eligibility cutoff, I estimate "sharp" RD's in both the receipt of the pension and labor supply, and then proceed to estimate a "fuzzy" RD to determine the causal effect of pension receipt on labor supply. For robustness, I estimate models using both linear and quadratic polynomials on either side of the age cutoff for various bandwidths. The model I estimate is described by the following:

$$y_i = \alpha + \beta \mathbf{1}[age_i > 65] + \gamma f(age_i - 65) + \varepsilon_i \tag{1.6}$$

[8] In addition to these concerns regarding the validity of the discontinuity, there may exist some concern about age "heaping" which has been identified (and addressed) in A'Hearn et al. (2009) as well as in other studies. It should be noted that age heaping is much less of a concern in this situation, as I look at age by month, and also remove those within a month of their birthdays. However, to this end, a table of observation counts by month of birth can be found in 1.2.

where y_i indicates the outcome for a household head, age_i indicates the age in months of the household head i, and $f(age_i - 65)$ indicates a polynomial of the running variable, which is the age in months of the respondent at the time of survey less the age in months at which the respondent will turn age 65. [9] β represents the RD estimate. The specification for the "fuzzy" RD estimate is identical, with the caveat that an additional OLS regression (for which I also include no additional fixed effects or regressors) provides the two stage least squares estimates. The OLS estimate is numerically equivalent to estimating two sharp RD estimates (one of the outcome and one of the instrumented variable) and taking their ratio. Because the RDD does not rely on fixed effects or covariates for proper estimation, I omit these for simplicity (Lee and Lemieux (2010a)).

1.4.3 Results

Tables 1.3, 1.4, and 1.5 show the main results. Table 1.3 shows results estimated using a flexible linear specification, 1.4 shows results estimated using a flexible quadratic polynomial specification, and Table 1.5 shows results estimated using Calonico et al. (2014a)'s bandwidth selection procedure and local linear regression around the cutoff. Results using Calonico et al. (2014a)'s procedure include a different sample than the others as the bandwidth selection does not rely on a given bandwidth. All three tables share the same set of columns, where each column represents a dependent variable.

According to the first column, eligible male heads are 8-10 percentage points more likely to receive a government pension, and are roughly 4 percentage points less likely to be employed.[10] The point estimates for labor force participation are similar to those for employment. These estimates represent "sharp" RD estimates and form the first stage and reduced form, respectively, for the "fuzzy" RD estimates presented in columns 4 and 5. These estimates assume that instead of eligibility, receipt of the government pension is the proper assignment variable and increases in probability on the right side of the age cutoff. The estimates are quite sizable: across all model specifications, they imply that those male heads receiving government pensions are about 45% more likely to report not working. Again, estimates for labor force participation are similar.

In addition to the numerical estimates shown in the above tables, I also show graphical evidence of the discontinuity in Figures 1.1, 1.2, and 1.3. Each figure shows monthly bin averages of the variable on the y-axis plotted against the relevant positive or negative age bin around the cutoff. Figure 1 displays the

[9] Note here that actually take advantage of the day of eligibility. To compute "months from eligibility," I take groups of 30 days on either side of the cutoff, so that the "months" I refer to here are not calendar months but rather groups of 30 days.

[10] It is important to keep in mind that while the estimate of 4 percentage points may not seem particularly striking, this is estimate is conditional the fact that roughly half of male urban workers are still working upon reaching age 65. A back of the envelope calculation involving the estimate of the constant term in Table 1.3 and the discontinuity yields a conditional estimate of roughly an 8% change in the likelihood of being employed upon reaching age 65 and still working.

probability of receiving a government pension against age, and figures 2 and 3 display the probability of reporting employment and labor force participation, respectively. Linear and quadratic polynomials for both sides of the cutoff are also transposed, however they are not drastically different and virtually lie on top of each other in Figures 2 and 3. Overall, the discontinuity in the three main variables of interest appears visually evident.

1.4.4 Differences in Labor Supply Responses

To determine if the response to pension eligibility differs by household composition, I group each household head according to his secondary education level and according to whether or not he lives alone. I then examine the behavior responses using methods similar to those I use above. Overall, my results are suggestive that those who live alone are more reactive to the pensions.

Tables 1.6 and 1.7 show separate results for both men who live alone and men who live in households with others. Men who live alone are over 80% likely to leave the workforce and labor force, upon receipt of the old age pension, compared to roughly 40% of men who live with at least one other individual. Both estimates are precisely estimated in all specifications, and are significantly different than each other in the linear case [11]

1.5 Factors Determining the Behavioral Response

I describe a model in which a substantial fraction of workers, facing credit constraints and generous pensions, act rationally by retiring when they become eligible. The assumption of credit constraints proves necessary in the model to generate behavior around the age of eligibility. The work of Friedman (1957) and Macurdy (1981) indicate that in the absence of anticipated changes in income are part of "permanent income" and should not be contemporaneous with changes in consumption or leisure, if agents are acting optimally. Given the results we have seen in a previous section, it seems the case that credit constraints must be present if we are to assume agents are acting rationally. [12] However, credit constraints them-

[11] The t-statistic of the difference using the linear specification is -1.981, and using the quadratic specification, -1.577.

[12] In the standard model of dynamic labor supply over the lifetime Macurdy (1981), changes in labor supply that are concurrent with changes in income must be due to either the change in income being unexpected or a lack of ability to redistribute future income over previous time periods which is usually accomplished via credit constraints. Of course, I cannot rule out that some individuals may simply not know about the pension program ahead of time, but given that (1) the program has been publicly addressed several times by politicians and (2) that the average life expectancy of Brazilians is around 70 years and hence most rational agents would be aware of any means of old-age support within a decade of their expected death, I do not consider that the program represents an unexpected income "shock." Additionally, the policy application of my results in-

selves are not sufficient for the observed response: the pension must be generous, too. This section will first formally show how the environment of borrowing constraints and generous pensions necessitates retirement "bunching." The final part of the section will examine heterogeneity in the response.

It is worth discussing exactly what I mean by credit constraints. In this paper, I will refer to credit constraints as constraints on those who want to borrow but are excluded from borrowing at an interest rate they would be willing to pay. I do not assume that those who want to save are barred from doing so; however, I do not believe savings to be a complicating factor.

1.6 A Model of Retirement "Bunching" Under Credit Constraints

1.6.1 Brief Description

This section describes an intuitive model in which workers choose consumption and when to retire facing an expected pension against which they are unable to borrow[13]. The model provides a rational decision-making framework which predicts the bunching evident in the data. Specifically, even in the face of no explicit incentive to retire, retiring *at* the age of eligibility becomes the optimal choice for more and more individuals as the size of the pension increases.

Suppose workers differ in the dimension of how they value leisure, and are the same otherwise. Workers value leisure at $\nu_{it} = \alpha_i + \mu * age$ and retirement is assumed to be irreversible. Specifically, retiring at age X provides utility $\sum_{t=X}^{T} \alpha_i + \mu t$. Workers maximize lifetime (log) utility, choosing consumption c and retirement age N facing a pension eligibility age of $\bar{N}$. They earn y for every period of labor and receive d as a pension after age $\bar{N}$. They can choose to retire at age N **before**, **after**, or **at** the age of eligibility, $\bar{N}$, and pick the age which provides the highest lifetime utility. For simplicity and clarity, I assume no time-discounting, and an interest rate of 0 for all borrowing and lending.

The worker's general lifetime utility maximization problem can be thought of:

$$\max_{N, c_{pre}(t), c_{post}(t)} Nu(c_{pre}(t)) + (T - N)u(c_{post}(t)) + \sum_{t=N}^{T} \alpha_i + \mu t \tag{1.7}$$

tends to explore what could potentially happen in other old-age social insurance systems, and the incidence of elderly workers simply not knowing about generous government benefits is not widely considered.

[13] The relaxation of the assumption of a total borrowing constraint to a parameterized borrowing constraint does not change the intuition behind or the conclusion of the model substantially.

where $c_{pre}(t)$ represents consumption before retirement, and $c_{post}(t)$ represents consumption after retirement.

Optimal consumption for workers results in an attempt to consume the same in every period. Maximizing lifetime utility with respect to retirement age N implies the following condition:

$$u'(c^*)y = \alpha_i + \mu N^* \tag{1.8}$$

This condition states that the marginal benefit of working one more period in the form of additional consumption must be equal to the value of one more time-unit of leisure. Note that the decision to work an additional period depends only on the return to working (i.e. income and the disutility of labor) and not on the size of the pension or other factors.

The model predicts that optimal retirement at $\bar{N}$ becomes the optimal choice for a range of disutility values as pension amount d increases. This is explained in more detail in the following section.

1.6.2 Maximization and Bunching

Recall that the worker's choices of when to retire can be classified into 3 "types": $N^* = \bar{N}$, $N^* < \bar{N}$ (bounded below by 0), and $N^* > \bar{N}$ (bounded above by T). $N^* > \bar{N}$ and $N^* < \bar{N}$ both satisfy maximums in their domains, and the worker picks the choice (either $\bar{N}$, $N|N > \bar{N}$, or $N|N < \bar{N}$) that provides the highest payoff.

Now, for notational clarity, if $N^* < \bar{N}$, then let $N^* = N^{*L}$, and if $N^* > \bar{N}$, then let $N^* = N^{*R}$; these can be thought of the pre-eligibility optimal retirement age and the post-eligibility optimal retirement age. Note that if the worker chooses to retire before $\bar{N}$, he will have to save for the gap of $(\bar{N} - N^{*L})$, and if he chooses to retire after $\bar{N}$, then he will smooth the additional income for the period that he is working and receiving the pension for a combined income of $y + d$.

Furthermore if $N^* = N^{*L}$, then it is the solution of the following problem

$$\max_{N, c_1^L, c_2^L} Nu(c_1) + (\bar{N} - N)u(c_2^L) + (T - \bar{N})u(c_3^L) + \sum_{t=N}^{T} \alpha_i + \mu t, \tag{1.9}$$

where

$$c_3^L = \frac{N(y - c_1^L) - (\bar{N} - N)c_2^L}{T - \bar{N}} + d \tag{1.10}$$

Also, if $N^* = N^{*R}$, then it follows that is the solution of the problem

$$\max_{N, c_1^R, c_2^R} \bar{N}u(c_1^R) + (N - \bar{N})u(c_2^R) + (T - N)u(c_3^R) + \sum_{t=N}^{T} \alpha_i + \mu t, \tag{1.11}$$

where

$$c_3^R = \frac{\bar{N}(y - c_1^R) - (N - \bar{N})(y + d - c_2^R)}{T - N} + d \tag{1.12}$$

In both cases, the necessary conditions for optimization remain the same: workers attempt to smooth consumption across all periods as much as possible, and equate the marginal value of working with the value of one time-unit of leisure, as in equation ((1.8)). Figures illustrating these problems are (1.4) and (1.5), for the pre-eligibility problem and the post-eligibility problem, respectively.

Payoffs are written below:

$$U(N^*) = \begin{cases} N^{*L}u(c_1^L) + (\bar{N} - N^{*L})u(c_2^L) + (T - \bar{N})u(c_3^L) + \sum_{t=N^{*L}}^{T} \alpha_i + \mu t \text{ if } 0 < N^* < \bar{N} \\ \bar{N}u(c_1^R) + (N^{*R} - \bar{N})u(c_2^R) + (T - \bar{N})u(c_3^R) + \sum_{t=N^{*R}}^{T} \alpha_i + \mu t \text{ if } \bar{N} < N^* < T \\ \bar{N}u(c_1^M) + (T - \bar{N})u(c_2^M) + \sum_{t=\bar{N}}^{T} \alpha_i + \mu t \text{ if } N^* = \bar{N} \end{cases} \tag{1.13}$$

The following theorem obtains in the solution to this problem.

Theorem 1 *As the value of the pension d increases, a choice of $N^* = \bar{N}$ is optimal for a larger range of values of leisure.*

The worker picks $N^* = \bar{N}$ as long as $U(N^* = \bar{N}) > U(N^* > \bar{N})$ and $U(N^* = \bar{N}) > U(N^* < \bar{N})$. This condition implies that α_i falls within certain bounds. Specifically, workers will pick $N^* = \bar{N}$ as long as:

$$\frac{T}{\bar{N} - N^{*L}} \left(u(\frac{\bar{N}y + (T - \bar{N})d}{T}) - u(\frac{N^{*L}y + (T - \bar{N})d}{T}) \right) - (\mu)(T)(0.5)$$
$$> \alpha_i >$$
$$\frac{T}{N^{*R} - \bar{N}} \left(u(\frac{N^{*R}y + (T - \bar{N})d}{T}) - u(\frac{\bar{N}y + (T - \bar{N})d}{T}) \right) - (\mu)(N^{*R})(0.5)$$

and these bounds *increase* in d, indicating that the larger the pension, the larger the range of α_i that satisfy this condition (hence, bunching occurs at larger d's). A proof is available in the appendix.

1.7 Brief Applications

The model I develop in the previous section is useful not only in explaining the bunching phenomenon, but also has applications in predicting economic behavior. In this section, I describe how the model I have developed can be used to firstly provide an estimate of the value of leisure (or the disutility of leisure) and secondly to provide an estimate of elderly aggregate labor supply.

1.7.1 Welfare Estimates

Note that a direct implication of this model is that the value of a year leisure for those who retire at the age of eligibility is bounded above and below by the aforementioned bounds. Therefore, given the value of pension d, the wage y along with institutional parameters such as $\bar{N}$ and life expectancy T, an individual (and aggregate) estimate of the value of leisure for elderly workers is available. The functions shown in figure 1.6 are correspondences between chosen retirement ages and the value of leisure, conditional on the value of the pension.

However, the degree of bunching at the age of eligibility taken in conjunction with estimates for the utility that should induce such bunching can be used to back out the underlying welfare function. Assuming a distribution of values of the disutility of labor across eligible workers and knowledge of average wages and pension payments, a bounding exercise (of aggregate welfare measures) can be performed.

Admittedly, any welfare exercise must take into account a considerable number of assumptions, and there are also not simple solutions for these bounds. I only seek to explain here that there *are* welfare implications to be drawn from this model, and a parameter of interest, specifically the disutility to an elderly worker of providing a year's worth of labor, can be estimated taking the model into account. Such a parameter may be of great interest to policy-makers attempting to incentivize the elderly to remain in the workforce.

1.7.2 Pensions and (Aggregate) Elderly Labor Force Participation

Of interest to policy-makers is how elderly labor force participation may change in response to financial incentives to retire. If a significant portion of the funding for the pension program comes from payroll tax contributions, such information may be particularly useful to policy-makers concerned with pay-as-you-go (PAYG) funding situations.

Assuming uniform and normal distributions of the value of leisure across the elderly population, a plot of the fraction of workers working at or past age 65 shown against the generosity of the pension d in figures 1.7 and 1.8. These figures also include separate plots of the fraction of workers working *past* age 65 and the fraction of workers working *at* age 65. These are equivalent to the cumulative density of workers over age 65 as a function of the pension generosity. These plots serve to highlight the information communicated in figure 1.6 about the distribution of elderly employment.

Note that there is considerable reshuffling of the elderly workforce as a result of pension generosity – as shown in 1.6, the distribution of the elderly workforce tends to shift "right" toward the age of eligibility. Note that as the replacement rate grows, the elderly employment rate drops – a larger replacement pushes individuals toward picking N^{*L}. However, at a sufficiently high replacement rate, the elderly employment rate levels off after it becomes optimal for *all* workers to retire upon reaching eligibility (and not earlier).

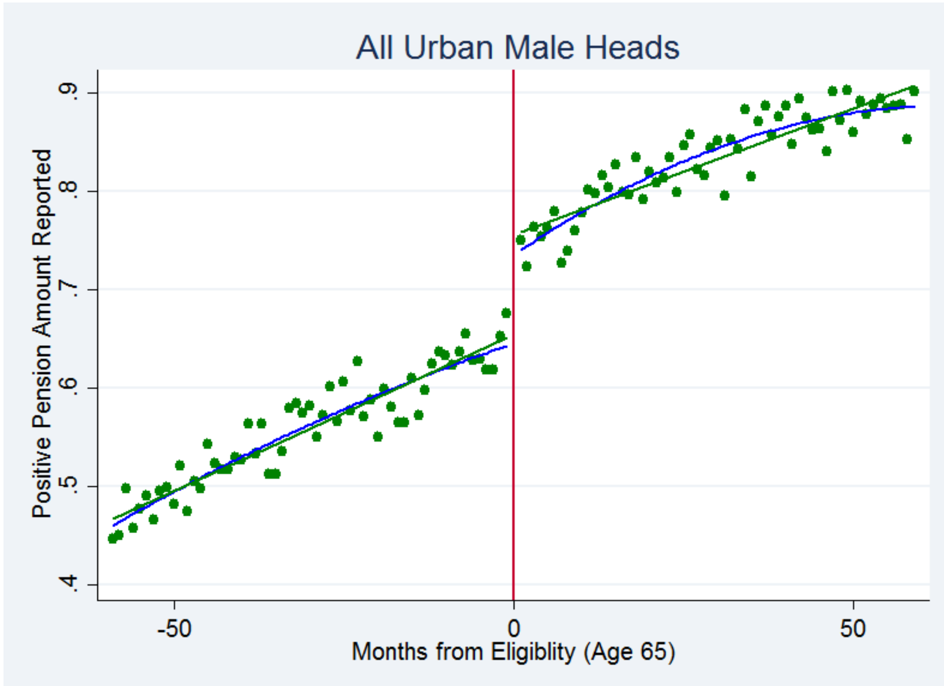
This exercise is done to highlight a central mechanism I outline in this paper – the notion that pension generosity, over and above the existence of credit constraints, can substantially incentivize retirement, yet also generate predictable variation in retirement ages.

1.8 Conclusion

This paper identifies a large behavioral response to pension eligibility that is associated with an increase in an individual's flow income. If agents are to be acting rationally in a life-cycle model of consumption, these results do necessitate credit constraints. However, I argue that credit constraints *alone* are not enough to explain the sharp degree of bunching evident in the data, and that another factor must be at work: specifically, the generosity of the pension. Specifically, I provide a model whereby extreme pension generosity including replacement rates of more than 100%, as is common in Brazil, can result in a point mass of individuals choosing to retire upon reaching the age of eligibility. Therefore, given the generosity of the pension, the mass of individuals retiring at the point of eligibility is expected.

My results are the first to document a contemporaneous behavioral response to pension eligibility at the monthly frequency – prior studies largely identify upticks in retirement at age groups but do not make finer distinctions than that. Moreover, I identify that the composition of households can affect the propensity to react to pension eligibility, which to my knowledge has not been documented in the litera-

Figure 1.1: Discontinuity in Probability of Receiving a Positive Pension Amount



ture. Finally, I propose a model which shows that the generosity of the pension can explain the bunching we see over and above the existence of credit constraints. I plan to use this model in future work to examine the welfare consequences in a more in-depth manner.

1.9 Figures

Figure 1.2: Discontinuity in Probability of Employment

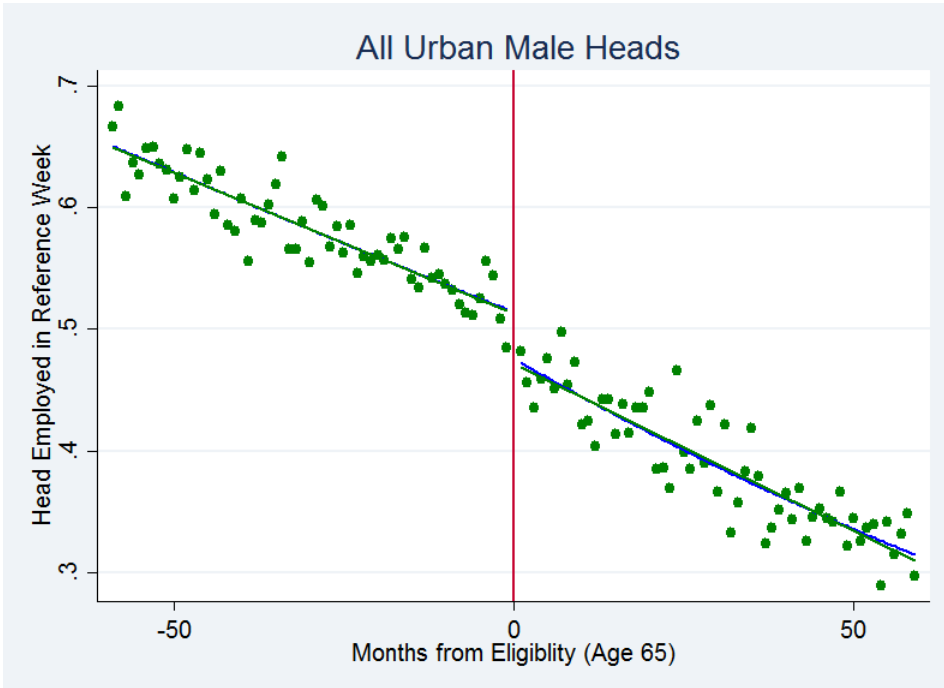
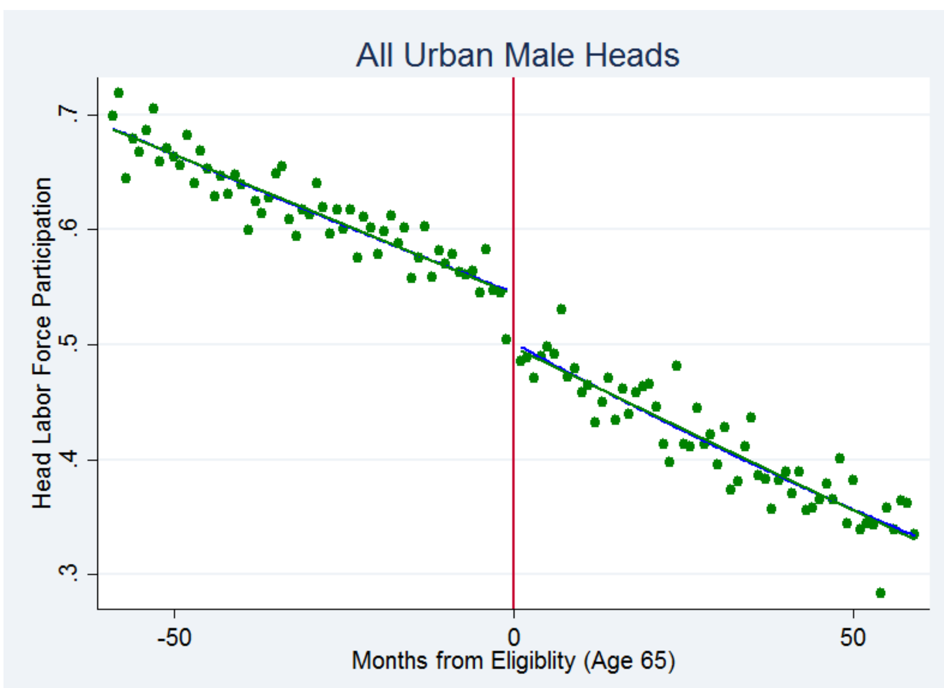


Figure 1.3: Discontinuity in Probability of Labor Force Participation



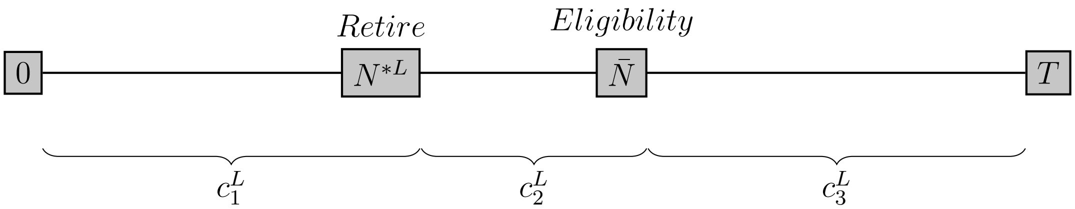


Figure 1.4: Pre-Eligibility Problem

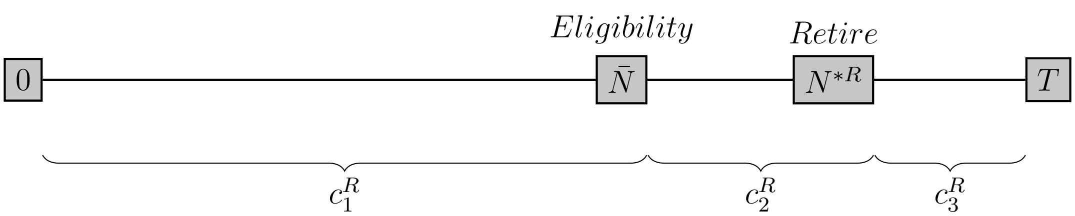
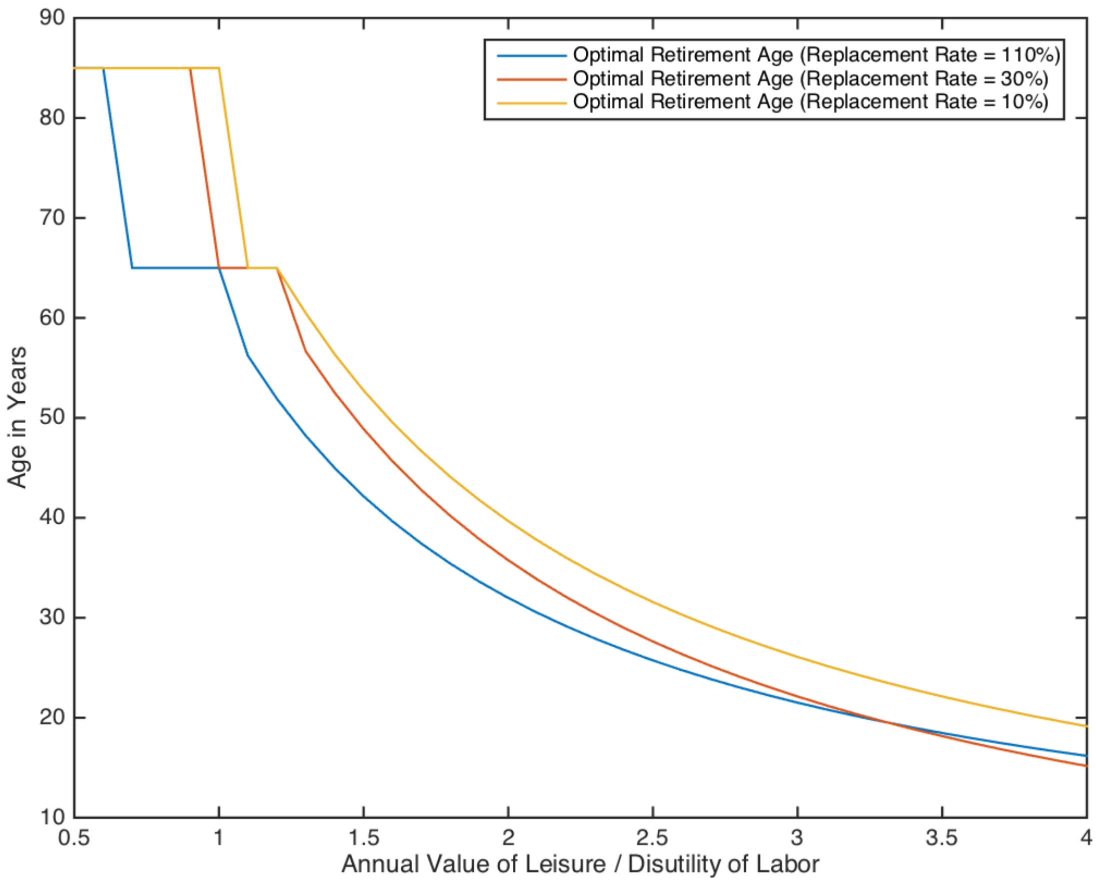


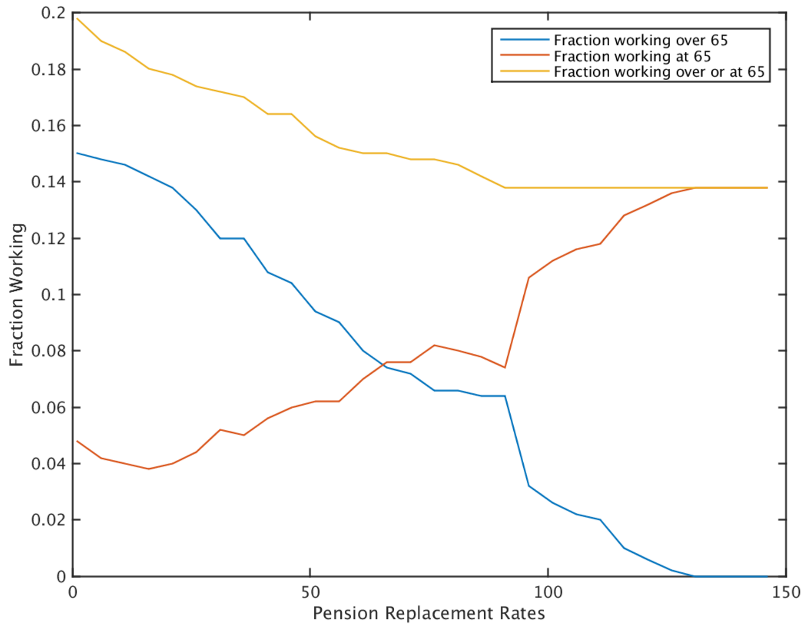
Figure 1.5: Post-Eligibility Problem

Figure 1.6: Optimal Retirement Ages for Pensions of Various Sizes



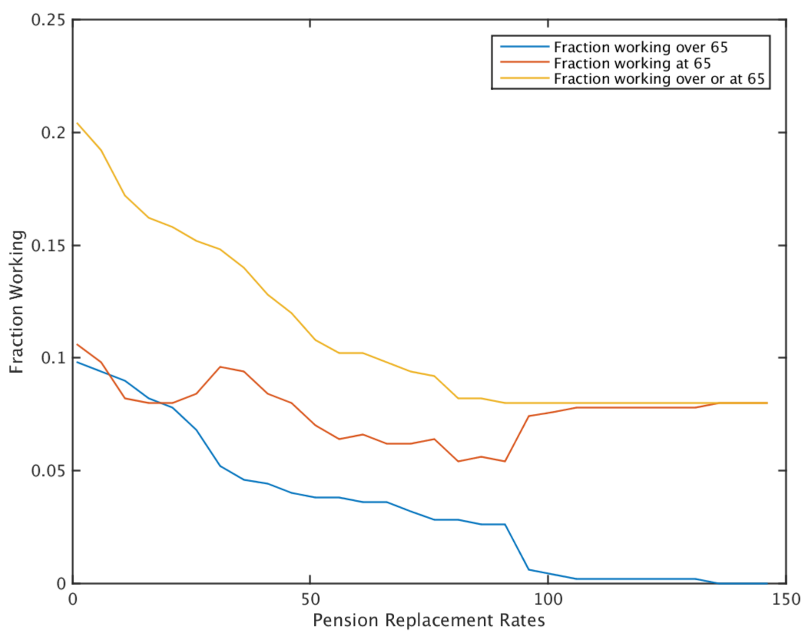
The model used to produce these optimal choices assumes log utility and that workers are unable to borrow any of the pension before becoming eligible. As mentioned, the future is not discounted and a real interest rate of 0 is used.

Figure 1.7: Employment Rates among Elderly Workers vs. Pension Replacement Rates (Uniform Distribution)



The model used to produce these optimal choices assumes log utility and that workers are unable to borrow any of the pension before becoming eligible. As mentioned, the future is not discounted and a real interest rate of 0 is used.

Figure 1.8: Employment Rates among Elderly Workers vs. Pension Replacement Rates (Normal Distribution)



The model used to produce these optimal choices assumes log utility and that workers are unable to borrow any of the pension before becoming eligible. As mentioned, the future is not discounted and a real interest rate of 0 is used.

Table 1.1: Descriptive Statistics of Analytical Sample (Dollar values in Year 2000 BRL)

	mean	sd
Number of People in Household	3.25	1.73
Household Monthly Income (Among Respondents)	1437.42	2231.08
Head's Personal Monthly Income (Among Respondents)	898.39	1745.01
Missing Household Monthly Income	0.04	0.20
Missing Head's Personal Monthly Income	0.03	0.16
Head with at least HS Education	0.13	0.34
Head with Less than HS Education	0.71	0.45
Head has More than Secondary Education	0.11	0.32
Head Literate	0.83	0.37
Spouse Literate	0.85	0.36
Head Received Government Pension >0	0.67	0.47
Total Weekly Hours Worked	41.63	15.92
Head in Labor Force	0.53	0.50
Spouse in Labor Force	0.37	0.48
Head Employed	0.51	0.50
Spouse Employed	0.35	0.48
Missing Head's Education	0.14	0.35
Missing Head Hours Worked	0.49	0.50
Observations	76255	

1.10 Tables

Table 1.2: Observation Counts by Month from Age 65 for Analytical Sample

Month from Age 65	Count
-9	633
-8	715
-7	673
-6	674
-5	593
-4	673
-3	572
-2	641
-1	682
1	660
2	645
3	672
4	623
5	667
6	673
7	613
8	624
9	593
10	568

Table 1.3: RD Estimates using a Linear Flexible Polynomial Specification

	All Urban Male Heads				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Pension >0	Employed	LFP	Employed	LFP
RD Estimate	0.106***	-0.0457***	-0.0466***		
	(0.00654)	(0.00718)	(0.00717)		
Pension >0				-0.430***	-0.438***
				(0.0621)	(0.0617)
Constant	0.641***	0.524***	0.544***	0.799***	0.825***
	(0.00516)	(0.00518)	(0.00513)	(0.0429)	(0.0425)
Observations	76,255	76,255	76,255	76,255	76,255
R-squared	0.097	0.045	0.049	0.196	0.206
idstat				253.5	253.5

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.4: RD Estimates using a Quadratic Flexible Polynomial Specification

	All Urban Male Heads				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Pension >0	Employed	LFP	Employed	LFP
RD Estimate	0.0950***	-0.0436***	-0.0434***		
	(0.0101)	(0.0110)	(0.0110)		
Pension >0				-0.458***	-0.456***
				(0.106)	(0.105)
Constant	0.631***	0.525***	0.545***	0.815***	0.832***
	(0.00770)	(0.00792)	(0.00788)	(0.0720)	(0.0711)
Observations	76,255	76,255	76,255	76,255	76,255
R-squared	0.097	0.045	0.049	0.196	0.205
idstat				87.45	87.45

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: RD Estimate using Bandwidth Selection via CCT (2014)

All Urban Male Heads

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Pension >0	Employed	LFP	Employed	LFP
RD_Estimate	0.0858***	-0.0420***	-0.0415***	-0.471***	-0.461***
	(0.0109)	(0.0109)	(0.0108)	(0.116)	(0.117)
Observations	173,802	173,802	173,802	173,802	173,802
Robust SE	0.0122	0.0129	0.0129	0.134	0.135
Eff. Obs to Left	19423	23032	23032	21577	20823
Eff. Obs. to Right	17714	20501	20501	19402	18851
LBW	29.39	34.49	34.72	32.28	31.73
RBW	29.39	34.49	34.72	32.28	31.73

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 1.6: Fuzzy RD Estimates: Single Heads

Single Male Head = 1				
	(1)	(2)	(3)	(4)
VARIABLES	Employed	LFP	Employed	LFP
Pension >0	-0.818***	-0.802***	-0.806***	-0.857***
	(0.197)	(0.198)	(0.270)	(0.276)
Constant	0.986***	0.990***	0.951***	0.995***
	(0.129)	(0.129)	(0.169)	(0.173)
Observations	7,643	7,643	7,643	7,643
R-squared	0.161	0.163	0.168	0.133
Linear Flexible Polynomial	yes	yes	no	no
Quadratic Flexible Polynomial	no	no	yes	yes
idstat	25.69	25.69	13.39	13.39

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Fuzzy RD Estimates: Heads with Other HH Members

Single Male Head = 0				
	(1)	(2)	(3)	(4)
VARIABLES	Employed	LFP	Employed	LFP
Pension >0	-0.378***	-0.389***	-0.397***	-0.386***
	(0.0658)	(0.0653)	(0.115)	(0.114)
Constant	0.771***	0.799***	0.782***	0.794***
	(0.0457)	(0.0453)	(0.0785)	(0.0775)
Observations	68,612	68,612	68,612	68,612
R-squared	0.189	0.200	0.190	0.199
Linear Flexible Polynomial	yes	yes	no	no
Quadratic Flexible Polynomial	no	no	yes	yes
idstat	229.1	229.1	76.20	76.20

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2

Local Government Spending and Employment: Regression Discontinuity Evidence from Brazil

This paper examines the causal effect of local government spending on labor markets in a developing country. We use plausibly exogenous variation in the allocation mechanism of federal funds at the municipality level in Brazil to estimate the effect of general spending on formal employment. We estimate that an additional 1% of spending at the local level (roughly $72,600) translates to an increase of about 20 formal jobs. This effect is much larger than other employment multipliers estimated in developed countries. We find that the size of the effect is driven by increases in employment from unskilled labor in the private sector, indicating that the transfer of federal funds can have a redistributive effect on the labor market, even if no such policy goal is explicitly stated.

2.1 Introduction

Fiscal policy is a primary means by which the government can affect the state of the economy and researchers often debate the effectiveness of government spending at creating jobs. Government spending can also entail vastly different outcomes for economies in developing countries. On one hand, corruption, differences in the local business environment, and the mistrust of government can all severely limit the extent of the influence of government spending in a developing country. On the other hand, fiscal policy can potentially be more effective in creating formal employment due to the lower cost of a job and higher unemployment and informality in developing countries. As a consequence of more elastic labor supply curves in the formal sector, small changes in the labor demand generated by government spending have the potential to create large formal employment increase in developing countries.

In this paper we estimate the causal effect of local government spending on labor outcomes in Brazil. A central challenge faced by researchers estimating the economic effects of fiscal policy is the lack of existing identifying variation in government spending. Governments often change their spending as a response to contemporaneous economic shocks. In order to address this endogeneity issue, we use a regression discontinuity design to isolate exogenous variation in local government spending, which we then use to estimate the effect of spending on local labor market outcomes. We exploit a discontinuity in the allocation of intergovernmental transfers: amounts of transfers from the federal government to municipalities under the *Fundo de Participao dos Municpios* (FPM) program vary according to population thresholds. Municipalities in the same state within the same population bracket receive the same amount of transfers. However, municipalities just above and just below the population bracket thresholds, which are presumably similar in observable and unobservable dimensions, receive significantly different FPM transfers.

We first demonstrate that FPM transfers translate into government spending. In our sample, municipalities just above the population cutoff spend on average 7% more than those just below the cutoff. Considering the incentives that municipalities have to obtain larger FPM transfers, we also investigate whether municipalities can precisely manipulate which side of the cutoff they may be on. We show evidence that municipalities had little power to determine their position around the population threshold during the period of our analysis.[1]

We focus mainly on formal employment using detailed data from an annual administrative survey covering the overwhelming majority of municipalities in Brazil. We estimate that an additional 1% of spending at the local level (roughly $72,604 on average) is associated with an increase of roughly 19.69 formal jobs in a municipality in a given year. This estimate can be translated to an estimated cost-per-formal-job-created of around $3,687 per year. We find that the majority of the job increase is composed of low-skilled and private-sector workers, and we do not find any detectable effects of government spending on average monthly wages.

In addition to estimating the impact of spending on employment and wages, we also investigate *how* municipal governments spend the "windfall" revenue associated with being above the population cutoff. Most of the spending is directed toward public investment rather than the public servant payroll: municipalities above the cutoff spend 14% more on public investments and only 6% more on the payroll of the municipal workers. We interpret this finding as consistent with the notion that mayors are forward-looking in the way they allocate government resources. Given that they are unable to lay off public servants or adjust wages downward in future periods, mayors might be discouraged to increase wages and employment in the public sector as a response to temporary positive revenue shocks. In contrast, public investments are more likely to generate better business environments and economic prosperity in the fu-

[1] As evidence for the validity of the regression discontinuity design, we find no evidence of bunching around the population cutoff for the years 2002 to 2007. However, we find evidence of bunching for the year 2008, when population was estimated by a pre-announced population recount. See section 2.3 for the details.

ture. The increase in public investment also explains the substantial increase in employment in the private sector, given that municipalities often hire private contractors for local construction projects.

This paper adds to a growing body of recent literature that uses novel instruments to identify exogenous variation in local spending in the US and has yielded fairly varied estimates. Shoag (2010) uses variation in government pension windfalls to estimate the impact of state government spending on the economy, estimating a cost per job created of around $35,000. Wilson (2012) uses state-level spending from the American Recovery and Reinvestment Act (ARRA) of 2009, instrumenting government spending with allocation formulas and pre-determined factors such as the number of highway lane-miles in a state or the share of youth in total population and estimating a $125,000 cost per job created. Chodorow-Reich et al. (2012) uses formula-driven variation in federal transfers to states in 2009 associated with state-level Medicaid spending patterns before the Great Recession, finding a cost per job created around $25,000. Finally, Serrato and Wingender (2014) uses variation in Census population counts to determine the allocation of government resources to estimate a cost per job of $30,000.[2]

While these findings are significant, this new literature on government spending has focused almost exclusively on developed countries.[3] Our results indicate that despite potentially important government failures, local government spending can be effective in creating formal jobs in developing countries. A possible explanation for the high job multiplier is the low productivity and associated low labor cost in Brazil, and indeed, the average monthly wage for the workers in our sample is only roughly 404 US dollars.[4] Nonetheless, higher unemployment rates and informality levels in Brazil might also be playing a role in government spending being even more effective in creating jobs, as it can generate a highly elastic labor supply of low-skilled workers to the formal sector.[5] As consequence, changes in the labor demand generated by an exogenous increase in government spending would lead to significant changes in formal employment and small changes in wages, which we observe in the data. While we cannot identify using available data whether these new jobs are associated with workers moving from unemployment or the informal sector, there is a substantial welfare gain for low-skilled workers to be employed in the formal sector.[6]

[2] Like our paper, most of these studies use windfall changes in revenue to examine how local government spending affects employment. However, most fiscal policy is implemented at the national level. The impact of fiscal policy at the national level might be mitigated by Ricardian equivalence as consumers might anticipate an increase in future taxes or interest rates.

[3] The literature on government spending in developing countries is scant, but existing studies do hint at the notion that developing country (GDP) multipliers are quite small. Kraay (2012, 2014) use variation in World Bank spending projects to gain leverage on identifying the effects of fiscal spending on GDP. Kraay (2012) fails to find multipliers significantly different from doer on a sample of 29 developing (and almost entirely African) countries, and Kraay (2014) finds multipliers in the range of 0.3 to 0.5 on a larger sample of 102 developing countries. Caselli and Michaels (2013) uses variation in oil output among Brazilian municipalities to investigate the effect of government spending on many welfare indicators. They find modest-to-undetectable effects on household income. None of these papers estimate the effect of government spending on labor market outcomes.

[4] As a reference, according to the American Social Security Administration website, the average annual wage of an American worker in 2005 was $36,953.

[5] According to the World Bank data, in 2005 the unemployment rate in Brazil was 9.5% and the unemployment rate in the US was 5.2%.

[6] Using 2010 Census data, we find that workers in the formal sector without a high school degree earn on average 40%

This paper also adds to the literature that studies the relationship between FPM discontinuities and municipal welfare measures. Litschig and Morrison (2013) use FPM cutoffs to identify the effects of government spending on long-term education and income outcomes over the period 1980-1991. The authors find that municipalities that received extra FPM funds over the period 1982-1985 benefited in terms of higher schooling and literacy rates and lower poverty rates in 1991. Brollo et al. (2013) uses the FPM discontinuities to examine the effect of additional government revenues on political corruption and on the quality of politicians. The authors estimate that the larger revenues generated by these transfers increase corruption and reduce the average education of candidates for mayor. Finally, Corbi et al. (2014) explore the FPM cutoffs in conjunction with luminosity data to estimate the effect of local government spending on GDP in Brazilian municipalities. Their point estimates for multipliers from various econometric specifications range from roughly 1.4 to 1.8.

This paper contributes to this literature in several capacities. While other studies have examined the effect of FPM transfers on education and poverty in the 1980's and political corruption and municipal GDP using more recent data, this paper looks at labor market outcomes. To our knowledge, this is the first study to present an estimate of how many formal jobs in Brazil can be created as a result of local government spending. This information is important because governments often use fiscal policy to affect weak labor markets. Additionally, this paper shows that municipalities spend most of the windfall revenue on public investment rather than payroll, which we interpret as mayors being forward looking on how they spend the temporary revenue. The increase in public investment implies that the majority of jobs are created in the private sector, demonstrating that rather than a crowd-out of private sector labor by local policy, we observe a crowd-in occurring as local governments often hire private companies to perform construction projects. Finally, this paper discusses the potential distributive effects of government spending. We find that virtually all the increase in employment is coming from workers with less than a high school degree, suggesting that local government spending has the potential to reduce inequalities in the labor market in Brazil.

Our paper is organized in the following way: Section 2.2 outlines the institutional background of the FPM program and the data we use for estimation. Section 2.3 discusses our estimation strategy and the validity of the regression discontinuity design. Section 2.4 presents our results, and Section 2.5 presents robustness checks. Section 2.6 concludes.

2.2 Institutional Background and Data Sources

The Brazilian government operates in a highly decentralized manner. The 26 states of Brazil are subdivided into over 5,500 municipalities, or *municpios*, which are the lowest level of governance. Local political power, including the allocation of government resources, is concentrated within the executive

more than those in the informal sector.

government of each of these municipalities. Each has a directly elected mayor, or *prefeito*, that has major influence over the distribution of municipal funds, along with an elected council, or *Cmara dos Vereadores*.

Municipality governments provide an array of public services and are funded heavily by revenue sharing programs at the state and federal levels. As mandated by the Brazilian Constitution, municipalities are responsible for urban planning, land development, public transportation, and garbage collection. In addition, municipalities share responsibilities with the states and the federal government in the provision of energy, housing, health care and education. Most of the education spending by municipal governments is in primary education. In terms of health provision, municipalities often maintain local clinics and smaller hospitals. Urban development includes constructing and maintaining urban infrastructures. Municipalities might also develop social assistance programs such as cash transfers to households (Caselli and Michaels, 2013). In order to finance their operation, municipalities collect taxes on services and property. However, such revenue is relatively limited, and municipalities are heavily dependent on federal and state government transfers. In our analytical sample, municipal tax revenues constitute about 12% of total revenues in contrast to roughly 37% from the FPM transfer program.[7]

The FPM program is funded by federal taxes and is redistributed to states and municipalities according to sharing rules. From the total revenues raised by the Income Tax and Industrial Product Tax, the federal government allocates 23% to the FPM program, which is distributed to each of the 26 states according to fixed state shares. Within each state, FPM funds are then distributed to municipalities according to FPM coefficients determined by population brackets.[8] Table 2.1 presents the population brackets and their corresponding coefficients.

To be precise, the amount of FPM transfers to a municipality i and state s is

$$FT_{i,s} = \frac{FPM_s \lambda_i}{\sum_{j \in s} \lambda_j}$$

where FPM_s is the amount of resources allocated to state s, and λ_i is the FPM coefficient of municipality i based on its population size and $\sum_{j \in s} \lambda_j$ is the some of all FPM coefficient in state s. Note that as each state receives a different FPM_s, two municipalities in the same population bracket receive identical transfers only if they are located in the same state. As for the designated use of these funds, the program stipulates that 30% of the funds must be spent on education and health, but 70% is unrestricted (Brollo et al., 2013).

We show the population distribution of municipalities (measured in 2004) and FPM cutoffs in Figure 2.1.

[7]From the reminder sources of revenues, 23% are transfers from the state government and 28% are other transfers from federal government different from FPM. None of these other transfers use population cutoffs.

[8]This scheme for linking transfers to population was decided during the dictatorship that came to power in 1964, in an effort to break with the clientele practice that transferred federal money to a narrow group of municipalities; Litschig (2012) has explored the history of the sharing rule in more detail.

As can be seen in the figure, most Brazilian municipalities are very small, reflecting a large amount of local political division. We find that the population associated with the first FPM cutoff is around the 49th percentile of the population distribution of the municipalities, meaning that 49% of all municipalities had a population smaller than 10,189. The second and third cutoff represent the 59th and 67th percentiles of the population distribution respectively.

In terms of municipalities finances, general expenditures (*despesas no financeiras*) are subdivided into personnel spending, capital spending and other spending. Personnel spending include salaries, benefits, allowances, retirement income and pensions. In our analytical sample, it comprises around 45% of general spending of which a large majority (around 85%) goes to the active workforce in our analytical sample. Capital spending is associated with an increase in municipal government physical capital. Other spending includes any other expenses that the municipal government might face in a year, such as the purchase of non-durable goods.

Additionally, the relationship between revenues and expenditures is particularly tight in Brazilian municipalities due to the enactment of the Fiscal Responsibility Law (LRF) in 2000, which strengthened fiscal institutions and currently requires the presentation of fiscal administration reports at four-month intervals with a detailed analysis of the government budget execution. The LRF has strict penalties if compliance is breached, including prison terms for public officials and suspension of election rights. Among the constraints included in the LRF are borrowing and debt limits, as well as caps for labor expenditures, making it harder for a municipality to smooth out a windfall in revenues, even if it expects revenue volatility in the future.

2.2.1 Data Sources

Our data come from three main sources. First, the population used for FPM determination is estimated by the Brazilian Institute of Geography and Statistics, or *Instituto Brasileiro de Geografia e Estatística* (IBGE). IBGE calculates the population in each municipality based on previous Censuses and population counts, birth and death rates, as well as migration trends. These population counts are provided annually to the public on the website of the Federal Court of Audits, or *Tribunal de Contas da Unio* (TCU), a federal accountability agency responsible for oversight of federally distributed funds. The estimates for the next year's FPM coefficients are published in October, a few months before the declaration of the municipal government budget.

Second, public finance data for Brazilian municipality come from the *Finanas do Brasil* (Finances of Brazil) annual survey, or FINBRA, of the Ministry of Finance. These dataset include information on revenues, expenditures, and FPM transfers by municipality and year. This data is available from the website of the Brazilian National Treasury (*Tesouro Nacional*). We are interested in general "non-finance" spending, which refers to all spending of a municipality in a given year with the exception of paying off interest or debts from previous loans. For our intents, non-finance spending will be henceforth referred to as "general spending".[9]

Third, the number of jobs and wages, by sector and by education, at the municipality level come from the Annual Report of Social Information, or *Relaao Anual de Informaoes Sociais* (RAIS). This data (aggregated at the municipality level) is publicly available from the website of the of the Ministry of Labor and consists of the universe of formal employees of the country. One important feature of the data is that the RAIS only covers workers in the formal sector. For our main analysis, we restrict the sample to full-time workers (those who work 31 or more hours per week) employed as of December 31st in the reference year.[10] The data contain consistent information at the municipality level over the years 2002 to 2010.[11] Finally, we use the 2000 Census to obtain characteristics of the municipalities in the sample that will be used as controls in some of our estimations. Henceforth, all monetary variables will be presented in year 2010 reais.[12]

2.2.2 Analytical Sample Construction

We make a few restrictions to the sample of municipalities that are used in the main analysis of this study. First, our study focuses on changes in government spending for municipalities around the first three population cutoffs: 10,189, 13,585, and 16,981 and we restrict our sample to observations within a +/-1,000 population bandwidth of these first cutoffs (we later show the sensitivity of our results to bandwidth choice, including using optimal bandwidths from Calonico et al., 2014b). While there are multiple population brackets used in allocating FPM funds, at subsequent cutoffs the variation in FPM transfers is too small to affect municipal overall revenues, and therefore there is no "first stage" in terms of overall resources available for the municipality (Litschig and Morrison 2013). As a robustness check, we present results using all eight population FPM. While we find a weaker relationship between FPM cutoffs and government spending, we estimate an even higher job multipliers in this expanded sample, although the effect is less precisely measured. Again, these first three cutoffs represent, respectively, the 49th, 59th and 67th percentiles of the population distribution of municipalities of Brazil around a high concentration of Brazilian municipalities (Figure 2.1).

The second main restriction is that we examine only the years 2002-2007 - we omit the last three years of data. In 2007, the government implemented a recount of the population in each municipality (*Contagem 2007*) in order to update the FPM coefficients starting in 2008. Other studies using the FPM transfers have noticed that in 2007 and Census years (years in which there was a headcount rather than an estimate

[9]"Finance" spending is a small fraction of overall spending (less than 1%) and does not jump at the cutoffs.

[10]As we do not observe individual identifiers and on the public version of RAIS (only job identifiers), by restricting the sample to those who work 31 or more hours, we ensure that we do not double count workers with two part-time jobs.

[11]Unfortunately, we cannot follow workers using the publicly-available RAIS data. As a result, we cannot test whether there is internal migration response to government spending using RAIS.

[12]In order to convert values from 2010 Reais to dollars, we use the average exchange rate of 2010 (0.57 dollars per 1 real).

of the population), there seems to be bunching just above the FPM cutoffs, and it is suspected that mayors of municipalities close to their respective cutoffs had the opportunity to manipulate their population to receive more transfers in the following year (a specific result is shown in Monasterio, 2013). The precise manipulation of the running variable around the population cutoff would invalidate our identification strategy. Therefore, we omit data following the recount between 2008 and 2010.

We also omit from our analysis municipality observations with abnormally spending amounts, which we characterize as 50% higher or lower than the municipality revenue. For our analytical sample, this corresponds to only 3 observations. Finally, we also omit from our analysis municipalities that have formal labor markets that are less than 0.1% or greater than 40% of the estimated population size (129 data points). These observations are likely errors from the administrative dataset and do not change our analysis substantively. After these sample restrictions, we end up with 1,223 municipalities and 4,842 observations[13].

The main characteristics of the sample are presented in Table 2.2. The average population size is 12,932. The FPM transfers correspond to about 38% of all revenues of the municipality, demonstrating the high dependency of municipal budgets on this type of transfer. The average spending of a municipality in a given year is about 12.73 millions of 2010 Reais, which is roughly equivalent to 7.26 million 2010 US dollars. Average spending and revenues are very similar, illustrating how tight the budgets are within the sample, as predicted by the Fiscal Responsibility Law, discussed in the previous section.

In our final analytical sample, the average total number of jobs in the formal sector is around 1,020 from which 44% are in the private sector, 42% in the public sector and 13% in other sectors (e.g. non-profit organizations). Most of the labor force in these municipalities is unskilled, with 62% of the jobs being filled by workers without a high school degree. The average monthly wage is 710 year-2010 Reais (404 US dollars), evidence of substantially lower labor costs in Brazil relative to those in the US.

2.3 Identification and Estimation Strategy

2.3.1 Validity of the Discontinuity

The main concern in estimating the effect of fiscal spending via a "naive" OLS approach is the potential bias of the estimate due to the implausibility of random government spending. For instance, government spending is often a *response* to economic outcomes and usually cannot be seen as random. In the regression discontinuity framework described above, our exogeneity comes from the assumption that observable and unobservable pre-treatment characteristics of municipalities are not discontinuous around the population cutoff. However, even in an RD environment, there can still be threats to this identification.

[13] We have an unbalanced panel as some municipalities leave the +/-1,000 population bandwidth overtime.

We identify two main sources of potential threats: (1) the exogeneity of the cutoffs, and (2) the manipulation of position around the cutoff. We argue that in our study, neither are cause for concern.

Exogeneity: Our design may be compromised if other programs involving municipality finances share the same population brackets as the FPM program; however, given the history of the determination of the cutoffs, this is not a cause for concern. As aforementioned, Litschig and Morrison (2013) note that the history of the seemingly arbitrary population bracket cutoff numbers originally come from the establishment of a redistribution program by a military junta in the 1960s aimed at allocating resources to areas by objective measures of need population happened to be a proxy for this. The original numbers were thought to have been multiples of 2000, however, they were subsequently updated with population counts and became the arbitrary numbers we see today. Given this history, it is unsurprising that no other known program uses these cutoffs.

Manipulation: If agents are able to precisely change their position around the cutoff in an RD design, the validity of the RD can be compromised. Population estimates in non-census years are estimated independently by the IBGE and then verified by Brazil's Federal Court of Audits (the TCU). As stated in the data description, the population estimates are based on previous censuses and population counts, birth and death rates, as well as migration trends. Mayors are never directly involved in the creation of population estimates. While we cannot rule out that the threat of some manipulation of these estimates remains, we find no empirical evidence of manipulation for the period 2002-2007. Specifically, using a McCrary (2008) test, we find no evidence of discontinuous breaks in the population density, for those years, as shown in Figure 2.2.

However, as we noted in the description of our analytical sample, there was a population recount in 2007 with the aim of correcting potentially erroneous groupings of municipalities into FPM population brackets. The recount was announced nationwide and mayors were aware that a higher population could be translated to a higher FPM transfers in the following year. A McCrary (2008) test in Figure 2.3 shows clear evidence of large breaks in the density of observations around the discontinuity for the population used for FPM transfers in 2008.[14] It seems clear, based on our results and others, that municipalities were (and are) somehow manipulating their position around the cutoffs in years in which the population is counted instead of estimated. There are various theories as to how (and by whom) such manipulation is taking place: mayors could be engaging in additional hiring in the year of the recount in order to artificially boost population, or be spending on amenities or incentives to attract potential citizens (and workers). To preserve exogeneity, we omit years 2008 and later from our analysis.

[14] Corbi et al. (2014) finds the same evidence of manipulation for the year 2008, and Monasterio (2013) has shown similar results for Census years.

2.3.2 Specifications

We estimate the effect of being just above the relevant threshold controlling for a polynomial in the running variable, including time and state fixed effects to "soak up" residual variation for municipalities within a given bandwidth of the population cutoff. Our specification follows the precedent of using a "polynomial" regression discontinuity estimator in the RD literature (Lee and Lemieux, 2010b and Imbens and Lemieux, 2008).

In order to pool the municipalities across the first three cutoffs, we follow the Litschig and Morrison (2013) estimation strategy: we first create a variable seg_{itj} that indicates whether the population of municipality i in year t is within a 1,000 bandwidth of the cutoff c_j:

$$seg_{itj} = 1[c_j - 1,000 \leq pop_{it} \leq c_j + 1,000] \text{ for } j = 1, 2{,}3$$

where c_j is the j's of the first three FPM cutoff presented in Table 2.1. As the distance between FPM cutoffs is always greater than 2,000, each municipality is unique to a segment seg_{itj} in a given year.

We then obtain the effect of being above a FPM cutoff on outcome Y_{it} for municipalities around the first three cutoffs by estimating the model:

$$Y_{it} = \beta \sum_{j=1}^{3} 1[pop_{it} \geq c_j] \times seg_{itj} + \sum_{j=1}^{3} g_j(pop_{it} - c_j) \times seg_{itj} + \sum_{j=1}^{3} \alpha_j seg_{itj} + \delta_t + \mu_s + \theta X_i + \varepsilon_{it}$$

where $g_j(.)$ is quadratic function with different slopes for $pop_{it} - c_j$ greater and lower than zero. This specification allows for different slopes for $g_j(.)$ at each cutoff level, controls for a population-bracket fixed-effect, and imposes a common effect β for all three pooled cutoffs. Year-fixed effects and state-fixed effects are captured by δ_t and μ_s respectively. Additionally, we also include time-invariant pre-treatment controls X_i (as measured by the 2000 Census) to soak up additional variation. In this estimation procedure, we only use observations of municipalities within a 1,000 bandwidth of the cutoffs c_j for $j = 1, 2, 3$. We test the sensitivity of our results to this bandwidth choice in section 2.5.2.

As in any RD design, our identification is based on the assumption that pre-treatment observable and unobservable municipality characteristics are continuously distributed around the threshold and therefore our identification strategy does not rely on covariates, year or state fixed effects. We include them only to eliminate small sample biases and improve the precision of estimates (Imbens and Lemieux, 2008). While for the main results of the paper we follow Brollo et al. (2013) and Litschig and Morrison (2013) and use state fixed effects and year fixed effects, we show in subsection 2.5.4 that our results are robust to the inclusion of state-year fixed effects and municipality fixed effects. We estimate Huber-White standard errors clustered at the municipality level.

2.4 Results

2.4.1 Government Revenues and Spending

Our identification relies on the discontinuity in FPM funds translating into higher government spending for municipalities above FPM cutoffs. For this purpose, we use detailed data from FINBRA on revenues and spending for each municipality in our sample during the period of analysis to determine how these funds influence spending.

We find that municipalities just above the first three cutoffs receive on average 14% more FPM transfers than those below the cutoff (Table 2.3). This result indicates that the mechanisms for FPM transfers are in accordance to what is established by law. We also find that the discontinuity in transfers translates to higher overall revenues for the municipality (Figure 2.4 and Table 2.3). We estimate that total revenues rise by 8%, on average, across FPM cutoffs. These findings suggest that municipalities are not able to fully compensate for revenue losses from being below the cutoff by increasing other sources of revenues, such as local taxes and other types of transfers. We indeed find no evidence discontinuities of other revenues around FPM cutoffs in section 2.5.

We also present evidence of the discontinuity in overall non-finance general expenditure around FPM cutoffs in Table 2.3 and in Figure 2.5. We estimate that government spending rises by 6.6% on average for municipalities across the cutoff, and the statistical significance of this estimate implies that the FPM population cutoff indicator is feasible as an instrument for government expenditure.[15]

2.4.2 Employment

We estimate that municipalities just above FPM cutoffs have on average 131 more jobs than those just below the cutoff, for an increase of around 8% (Table 2.4).[16] Note that while the graphical evidence for the discontinuity in employment appears noisy, much of our finding is driven by observations located extremely close to the threshold. Accordingly, the size of our estimates increases as we move closer and closer to the cutoff.[17] These large estimate hides substantial heterogeneity: specifically, this increase in jobs is concentrated in certain skill segments of the labor market. Employment increases are significantly higher for those without college degrees. Jobs involving college-educated workers increase almost negligibly, while jobs involving those without a secondary (high-school) diploma and those who only have a

[15] The F-statistic for this regression is 12.32.

[16] Figure 2.6 presents similar evidence for the discontinuity of employment around the cutoff.

[17] In our discontinuity graphs, we plot the residuals of the outcome variables, conditional on year and state fixed effects and selected covariates. As employment generally varies substantially across these dimensions, we remove these as determinants of employment and plot the residuals. The reason we do this is to reduce the apparent variance in the graphical estimates. A discontinuity graph with the original outcomes is available under request.

secondary diploma increase by around 110 and 14 respectively, accounting for nearly all of the increase in overall jobs.

Given the characteristics of the municipalities of the sample, it is perhaps not surprising that there is some increase in the amount of unskilled formal labor. As presented in Table 2.2, most of the labor force in these small municipalities is low-skilled. However, estimating the same regressions with a log transformation shows a large significant increase in percentage as well as in levels (Table 2.5). Low-skilled labor increased by roughly 15%, substantially more than did labor among those with secondary degrees or higher. Therefore, even conditional on the larger amount of unskilled labor in our sample, unskilled labor increased proportionally more than relatively more skilled labor. [18]

The "source" of these low-skilled workers is not quite clear, and unfortunately our data are not informative in this dimension. These municipalities have a significant number of less-educated workers who are unemployed and in the informal sector, and as the economy grows with government spending, it is possible that these workers migrate to the formal jobs (Ulyssea, 2014). Moreover, as we will describe shortly, the increase in government spending is concentrated in public investments, which are often associated with highly intensive in low-skilled labor construction projects (David and Dorn, 2013).

There also exists heterogeneity in the estimates by sector. Most of the job growth is concentrated in the private sector (Table 2.6). This indicates that rather than a crowd-out of private sector labor by fiscal policy, we observe a crowd-*in* occurring. Though puzzling, the nature of our quasi-experiment and the characteristics of the Brazilian labor market can offer an explanation these findings. Mayors may understand that being above the population cutoff might be an one-period, or temporary, revenue windfall, as they are not guaranteed the same level of transfers in the following year. Hiring public workers is necessarily associated with a higher payroll for an undetermined number of years, as public workers cannot be laid off in Brazil (Braga et al., 2009). As a consequence, mayors might choose to spend the extra revenue associated with being above the cutoff with private contractors, which seems to be consistent with our findings when looking at the type of government spending associated with extra FPM revenues in subsection 2.4.5. If true, this notion might offer some evidence as to how governments make hiring decisions in the face of uncertainty about the receipt of future funds.

2.4.3 Average Monthly Wages

We find small and insignificant effects of being above FPM cutoffs on wages by virtually all education levels and sectors (Tables 2.7 and 2.8) We interpret this result as evidence of the existence of a large supply of unskilled workers that are either unemployed or in the informal sector, which imply a very elastic labor supply curve in the formal sector in Brazil. As a result, labor demand shocks associated

[18] Note that the observation counts within skill groups are different than our total sample. We omit municipality-years from these analyses that contain no formal workers in these categories, owing these to administrative errors.

with the increase in government spending must generate small changes in wages and large increases in employment. Consequently, labor markets in Brazil become more responsive to government spending than the US. Nonetheless, we cannot rule out other potential explanations, such as that the rigid wage setting in Brazil implies that adjustments may happen more slowly or that internal migration can might mitigate any permanent wage premiums across municipalities (Arbache, 2001).[19]

2.4.4 Two-Stage Least Squares Estimates

In order to obtain a causal estimate, we examine the effect of local government spending on labor market outcomes using two-stage least squares estimation. In the first stage, we use the indicator for a municipality being above the cutoff as an instrument for the natural log of government expenditure. In the second stage we regress this instrumented log spending on the number of jobs and log average wages in each municipality. Using this method, we find that a 1% increase in government spending is associated with the creation of about 19.69 jobs (Table 2.9). Given that the average general expenditure in a municipality is roughly $7.26 million in our analytical sample,we calculate an average cost-per-job of around $3,687. As expected with the reduced form average monthly wage results, we do not find any evidence of an effect of spending on monthly wages.

Based on this estimation, we can test whether costs-per-job estimates are statistically different from those in the US literature. Wilson (2012) estimates a cost of around $125,000 per job, Chodorow-Reich et al. (2012) find a cost per job created around $25,000, and Serrato and Wingender (2014) estimate to be around $35,000. Based our the instrument variable regression, we estimate with 95% confidence an upper bound of $15,364 for the costs-per-job created in our sample. We conclude that the cost-per-job in Brazil is significantly lower than what the previous literature indicates for the US.

A possible explanation for the higher jobs multiplier is that labor is simply overall cheaper in Brazil than it is in the United States. Indeed, the average monthly wage in our sample is about roughly $407, much lower than in the United States (average monthly wage of worker in the US was $3,080 in 2005).[20] However, while we find that American wages are 7.5 times higher than the wages of workers in our sample, we estimate a jobs multiplier at least 10 times higher in Brazil than in the US. We conclude that the large number of unemployed and informal workers in Brazil is part of the explanation for the high job multipliers in the country.

The result of higher unskilled labor as a function of locally-allocated transfers and spending indicates that unrestricted government spending can have an implicit (or unstated) redistributive effect even if no such explicit policy goal is specified. If those from higher parts of the wealth distribution pay the

[19] As we cannot follow workers in the public available version of RAIS, we cannot rule out that workers are migrating from other regions.

[20] This estimation is based on the $36,953 average annual wage of an American worker in 2005, which is available on the American Social Security Administration website.

majority of FPM funds, and these funds then are used to hire workers from the lower end of the wealth distribution, the official government spending in some sense represents an unofficial transfer from one end of the distribution to the other. It is important to note that because these results are in the context of local government, it may not be the case that these results would "scale up" to the federal level; federal spending programs may be different in the kinds of spending they incur and may employ from different parts of the wage distribution. However, our result provides evidence that largely unrestricted transfers and spending at the local level translate to the hiring of more unskilled labor, resulting in presumed welfare gains for those hired and a potentially inequality-reducing redistributive effect.

2.4.5 The Distribution of Spending

The FINBRA data allows for the breakdown of public expenditure by type of spending. We divide general expenditure into 3 broad categories: personnel spending, investments, and other spending. Personnel spending corresponds to total spending on public servants: either active, inactive or pensioners. It includes salaries, benefits, allowances, retirement income and pensions. Investments are expenses necessary for the execution of construction projects and for the purchase of the facilities, equipment and permanent material. These expenses must be associated with an increase in municipal government capital. Finally, other spending includes any other expenses that the municipal government might face in a year, such as the purchase of non-durable goods.

The most substantial percentage increase associated with the FPM transfers is investment (Table 2.10). Municipalities just above the population cutoff spend 14% more on investment. In contrast, we estimate very modest increases in personnel spending. We estimate a 6% increase in personnel spending for municipalities above the cutoff. Finally, we also find significant effects on other spending (5%).[21]

We interpret the substantial increase in investment and the modest increase in personnel spending as mayors being forward-looking in the way they allocate government resources. On one hand, an increase in payroll might be unsustainable in the future as as a municipality might fall below the cutoff in the following year, and as we have noted before, it is difficult for mayors to lay off public servants or adjust wages downward. On the other hand, public investments are more likely to represent a kind of long-term source of revenue in that they may generate a better business environment, which might itself increase future tax revenues.

[21] We also find stronger effects in levels of spending (absolute terms) of being above the cutoff on investment than personnel spending. Precisely, we estimate that that municipalities just above the population cutoff spend about 334 thousand Reais in investment and 317 thousand Reais more on personnel spending than municipalities just below the cutoff.

2.5 Robustness Checks

2.5.1 Other Sources of Revenues

A potential concern of papers that explore "windfall"-type revenue shocks is that governments may find ways to adjust for the unexpected loss of revenue by increasing other sources of revenues. In order to address this issue, we investigate whether municipalities that are below the population cutoff are able to increase their own tax revenues or whether they receive other types of transfers from federal and state governments.

We explore whether there is any discontinuity around the cutoff on proprietary revenues and taxes and other transfers. Proprietary revenues and taxes are composed primarily of revenues from the Territorial Urban Property Tax (IPTU), and Services Tax (ISS), which are the two main municipal taxes. Other transfers correspond to shares of other state and federal taxes to which municipalities are entitled. None of these other transfers use an allocation mechanism based on the FPM coefficients.

We do not find evidence of a decrease in the sum of these other sources of revenues around the population cutoff (Table 2.11). Indeed, we find null effects of being above the population cutoff on both for both own taxes and transfers revenues. This result is not surprising given the very limited ability that municipalities have to increase their own revenues in Brazil since the enactment of the Fiscal Responsibility Law.

2.5.2 Bandwidth Selection

Another potential concern in any regression discontinuity design is whether the results are sensitive to bandwidth choice. In most of the analysis of this paper, we restrict the sample to municipalities that are within a 1,000 population bandwidth of the first three FPM cutoff. We examine the sensitivity of our main results to bandwidth choice, varying it from 250 to 1,500 and present our results in Table 2.12 .

Overall, we find that the employment effects vary little with bandwidth choice, with the highest estimate of an increase of about 270 workers with the tightest bandwidth selection of 250 people on either side of the cutoff and the lowest estimate of 93 workers with a bandwidth of 1,250. In all cases we find significant effects of FPM transfers on employment at at least the 10% level of significance. We also find that the lack of detectable effect of government spending on average monthly wages is not sensitive to the choice of the bandwidth, with coefficients varying from -0.008 to 0.019.

Finally, we use the method of Calonico et al. (2014b) to select the optimal bandwidths for local quadratic estimation (Table 2.13). We find greater discontinuity effects on employment using Calonico et al. (2014b) bandwidths than those estimated in Table 2.4. Nonetheless, following the suggested method, we do not include any covariates, year or state fixed effects in the estimations in 2.13, generating greater standard errors and therefore lower significance levels.

2.5.3 Placebo Cutoffs

We also undertake a "placebo" exercise in which we estimate the model at false FPM population cutoffs (Table 2.14). We consider placebo thresholds of 11,887; 15,283 and 20,377. These cutoffs correspond to the mid points of the first three brackets of FPM transfers (Table 2.1). In these estimations, we use the same bandwidth (1,000 population count) and specification from the ones used in the main analysis of the paper. The estimated discontinuity at the placebo cutoffs of 11,887; 15,283 and 20,377 on employment are indistinguishable from zero.

2.5.4 Municipality and State-Year Fixed Effects

As in any RD design, our identification is based on the assumption that pre-treatment observable and unobservable municipality characteristics are continuously distributed around the threshold. Neither covariates, year nor state fixed effects are needed for identification and we include them only to eliminate small sample biases and improve the precision of estimates.[22] In this subsection, we show that our results are also robust to the inclusion of municipality-fixed effects and state-year fixed effects. In the former, identification comes through municipalities crossing population thresholds during the period of analysis. The later specification is justified by the fact that FPM transfers received by each municipality varies by state-year.

In Table 2.15 we estimate that a 1% increase in government spending is associated with an increase of about 17.47 jobs in the in the state-year fixed estimation and 10.03 in the municipality fixed-effects specification. In both cases, we cannot reject at the 95% level that these job multipliers are different from 19.68 job multiplier we estimate in Table 2.9. As in the rest of the paper, we do not find any effect of government spending on average wages in those specifications. These results reassure the validity of our RD identification assumption.

2.6 Conclusion

We estimate sizable effects of government spending on employment in Brazil. Using our preferred estimation procedure, we find that a 1% increase in government spending is associated with an increase of about 19.68 jobs in the formal sector. This effect is much larger than other employment multipliers estimated in developed countries. We also fail to find any effect of government spending on average wages

[22] Litschig and Morrison (2013) and Brollo et al. (2013) use state fixed effects in their specifications and Corbi et al. (2014) uses municipality fixed effects.

in virtually all estimations. This result is consistent with a theory that higher levels of unemployment and informal sector in a developing country generate a very elastic labor supply of workers to the formal sector. As a result, labor demand shocks generated by government spending cause a higher increases in employment and little change in wages.

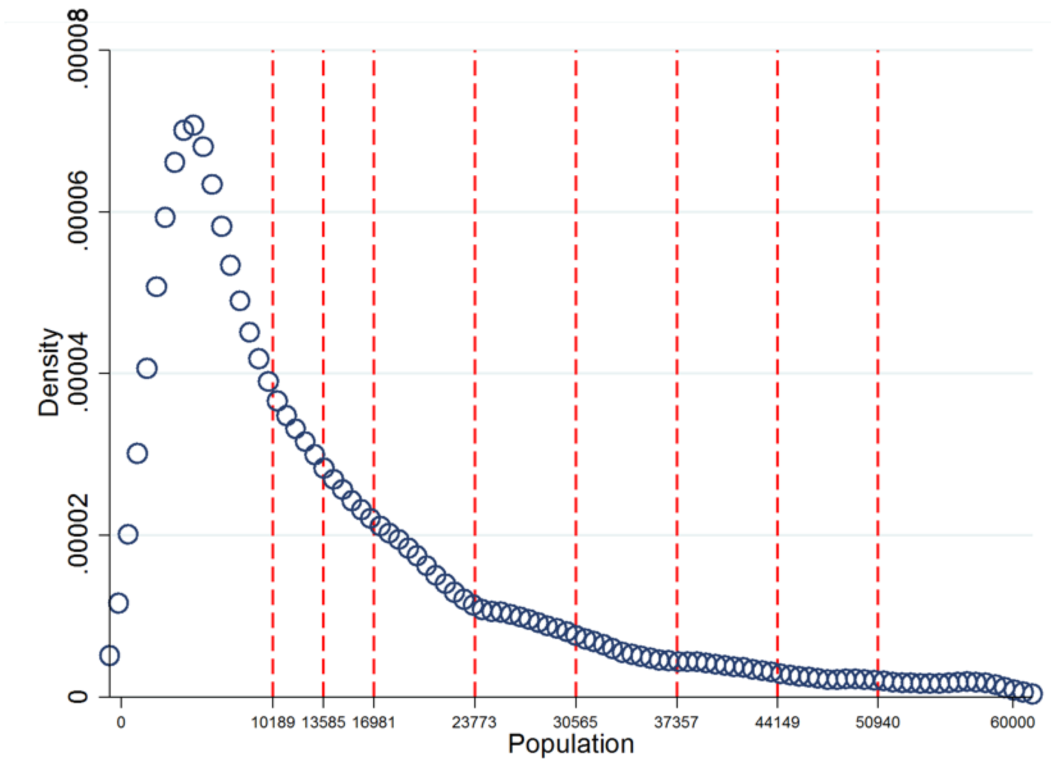
We also find that most of the job increase is concentrated among the lower end of the skill distribution and in the private sector. Additionally, certain types of spending increase, proportionally, more than others: namely, investment spending is increasing proportionally more than payroll spending. These results potentially suggest that local governments in Brazil act in a forward-looking manner in the allocation of government resources. Instead of hiring public sector workers, local governments seem to be increasingly spending their resources on public investments that might generate future economic growth. The high investment also explains the substantial increase in private sector jobs, as municipalities tend to rely on private contractors for this type of job.

As stated above, we find large employment effects among the lower-skilled segments of the workforce. To the extent that skill level is correlated with household income, our results indicate that local government spending may have a potential redistributive effect where one is not necessarily intended. Because the spending is largely unrestricted, our finding has potential implications for the allocation of both federal and local funds in the developing world. Our future research agenda involves the examination of long term effects of government spending on employment and income inequality.

Additionally, the effects of fiscal spending may not be limited to the localities in which the funds are actually disbursed. In future work, we plan to examine geographic mobility of workers across labor markets in response to government spending by examining how these FPM transfers affect the labor markets of neighboring (but not directly affected) municipalities. We believe that providing some bounds on how flexible the labor market in a country like Brazil will be of great interest to those interested in the unintended consequences of fiscal policy. Finally, it is important to note that our paper uses windfall changes in revenue to examine how local government spending affects employment. However, most fiscal policy is implemented at the national level where these effects could be mitigated by Ricardian equivalence, as consumers might anticipate an increase in future taxes or interest rates as a response to more expenditure.

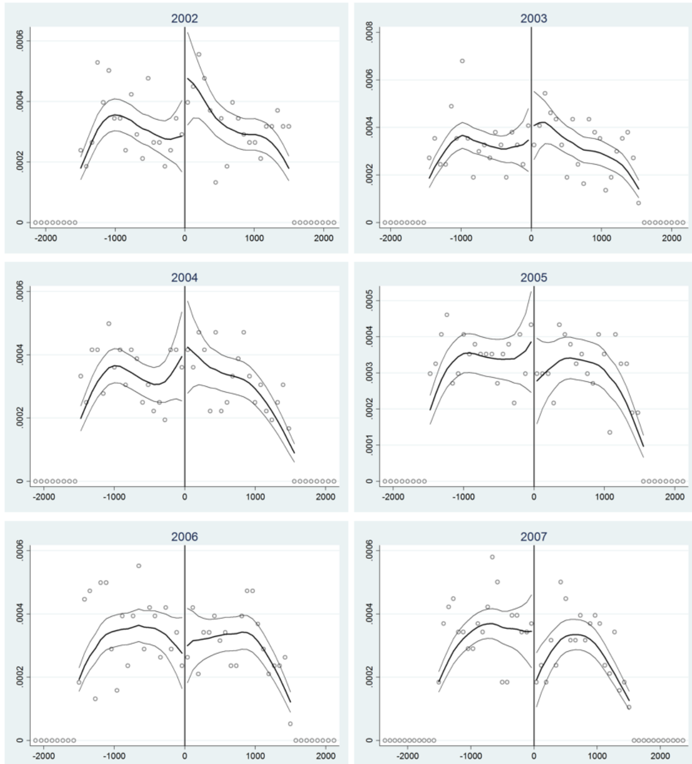
2.7 Figures

Figure 2.1: Population Distribution of Municipalities - 2004



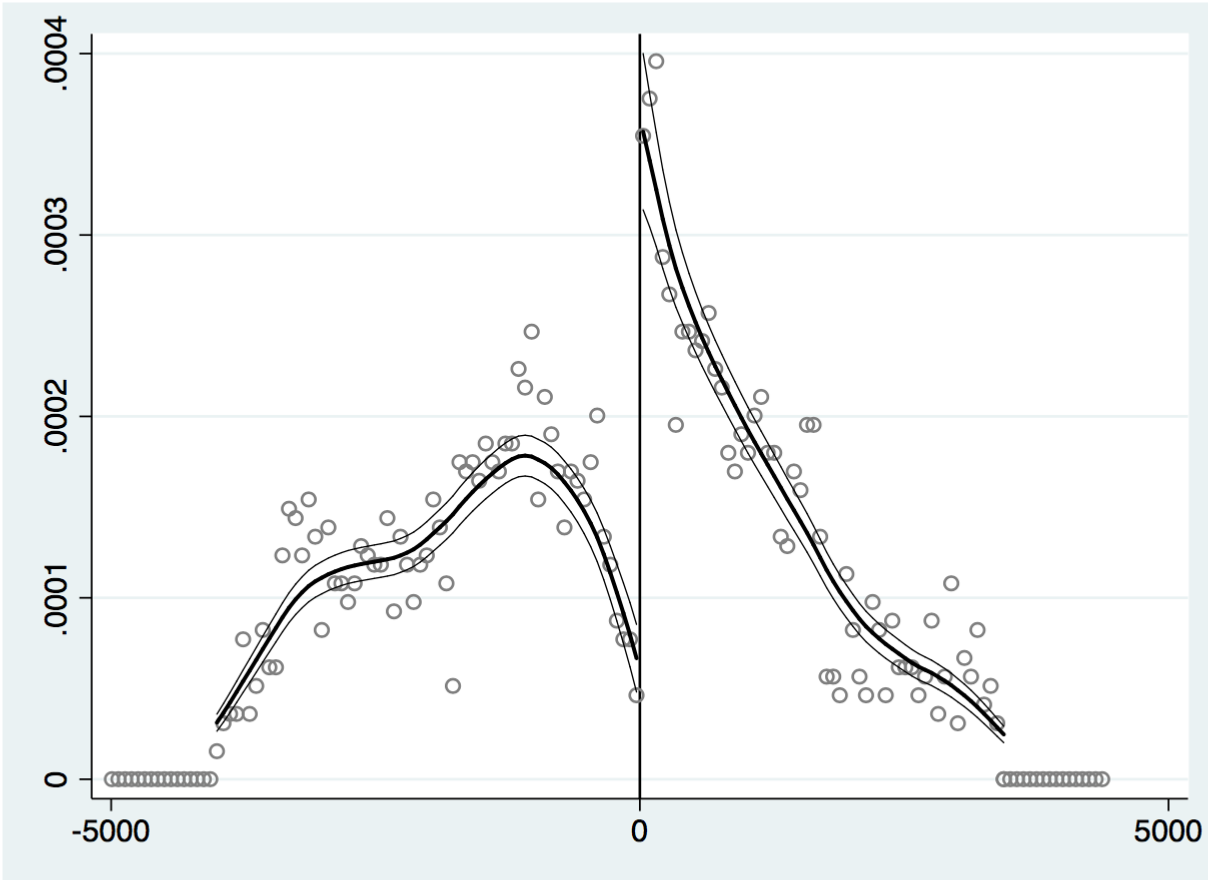
Note: Open circles represent the kernel density of the population distribution of municipalities in 2004, right truncated at 65,000 and using a 100 binwidth. The dotted red lines represent the eight FPM cutoffs.

Figure 2.2: McCrary Test, 2002-2007



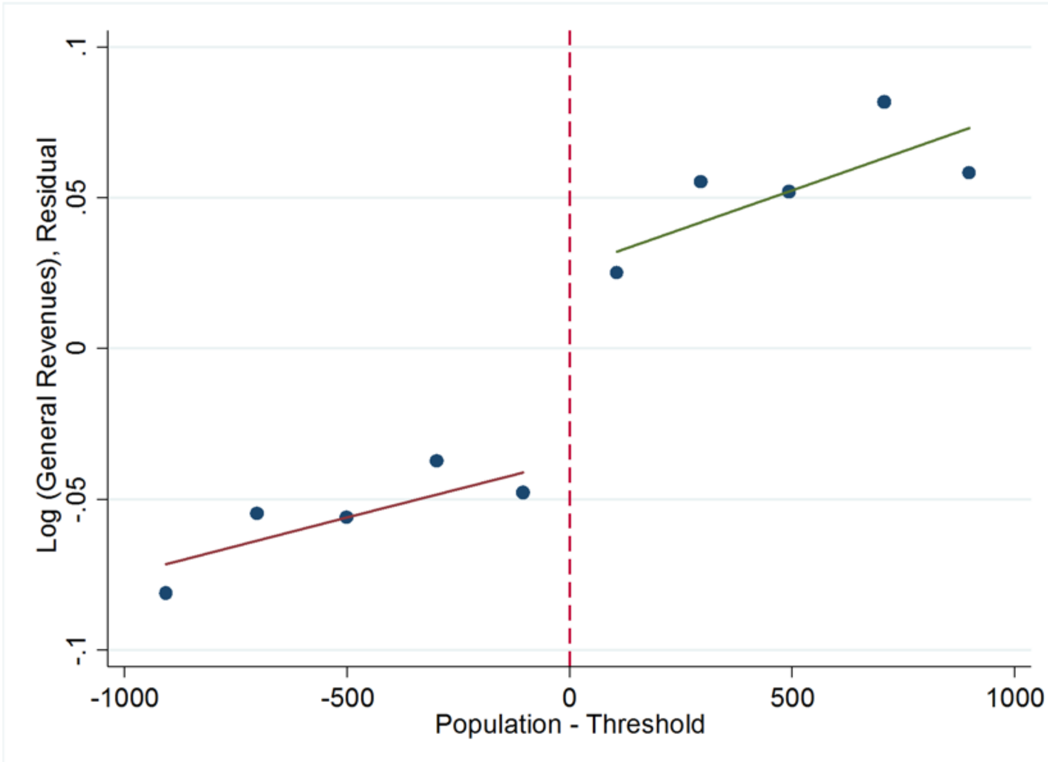
Note: Weighted kernel estimation of the log density of the distance to FPM cutoff performed separately on either side of the threshold. Optimal bandwidth and binsize as in McCrary (2008). Sample is restricted to municipalities with population with 1,500 inhabitants above and below the eight FPM cutoff. Population in 2002-2007 is estimated based on old Census, birth and death rates, projection of migration trends.

Figure 2.3: McCrary Test, 2008



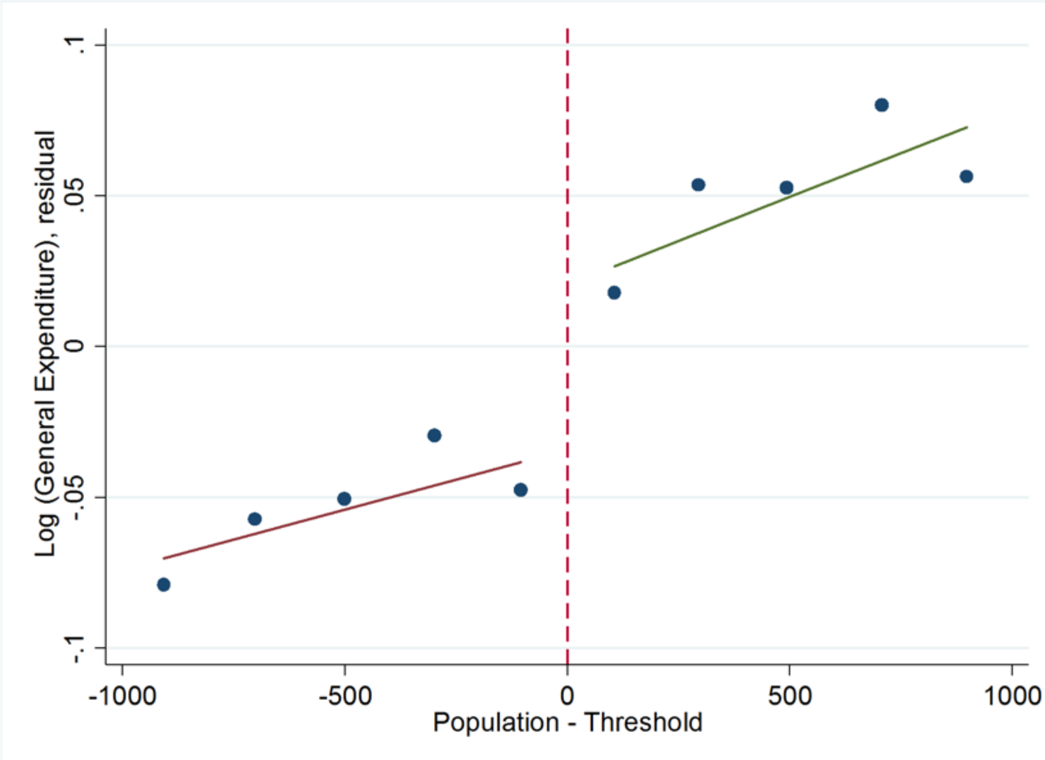
Note: Weighted kernel estimation of the log density of the distance to FPM cutoff performed separately on either side of the threshold. Optimal bandwidth and binsize as in McCrary (2008). Sample is restricted to municipalities with population with 1,500 inhabitants above and below the eight FPM cutoff. Population in 2008 was estimated based on the Recount of the population in 2007.

Figure 2.4: Ln(General Revenue) as a Function of Normalized Population



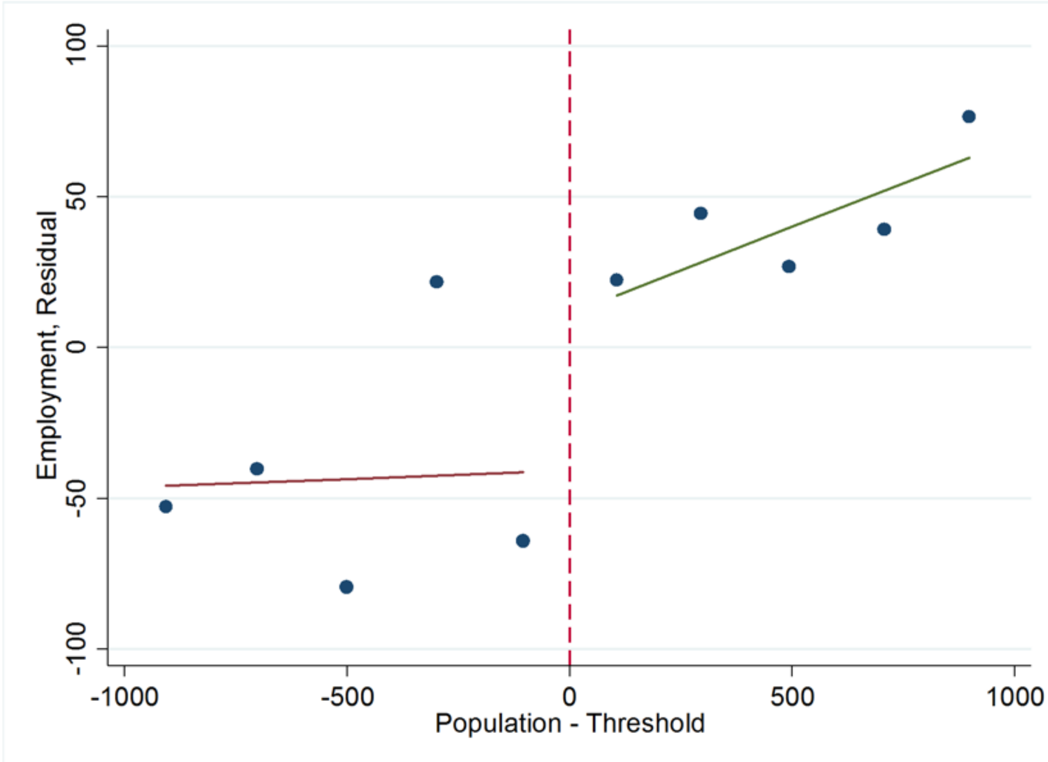
Note: Log (General Revenues), Residual is obtained from a regression of general revenues on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Figure 2.5: Ln(General Expenditure) as a Function of Normalized Population



Note: Log (General Expenditure), Residual is obtained from a regression of general expenditure on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Figure 2.6: Total Employment as a Function of Normalized Population



Note: Employment, Residual is obtained from a regression of total employment on year fixed effects, state fixed effects and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). We use residuals to make the local average graphs less nosier and consistent with our preferred specification. The discontinuity graph with the original outcomes is available under request. Open circles represent 200 population local averages and the lines are local linear fits below and above the FPM cutoff. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

2.8 Tables

Table 2.1: FPM Coefficients

Population Interval	FPM Coefficient
Below 10,189	0.6
10,18913,584	0.8
13,58516,980	1
16,98123,772	1.2
23,77330,564	1.4
30,56537,356	1.6
37,35744,148	1.8
44,14950,940	2
Above 50,940	from 2.2 to 4

Note: FPM coefficient is the coefficient used in the determination of FPM transfers. Total FPM transfers depend also on the state where municipality is located as well as the federal government tax revenues in a given year.

Table 2.2: Descriptive Statistics

Variable	Mean	Standard Deviation
Population	12,932	2,770
Municipality Finance		
FPM Transfers (1000s of Reais)	4,945	1,362
General Revenues (1000s of Reais)	12,949	7,275
General Expenditures (1000s of Reais)	12,737	7,224
Share of Governement Expenditure by Type		
Personnel Spending	44.5%	
Investment	10.9%	
Other Spending	44.6%	
Employment		
Overall Number of Jobs	1,020	935
Share of Jobs by Sector		
Private	44.2%	
Public	42.5%	
Other	13.3%	
Education		
Less than HS	62.3%	
HS grad	29.5%	
Some college	2.4%	
College grad	5.8%	
Average Monthly Wages by Sector (in Reais)		
Overall	710	235
Private	674	271
Public	907	541
Other	573	215
Observations	4,842	

Note: Monetary variables are measured in year 2010 Reais. The average exchange rate in 2010 is 0.57 dollars per real. Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007.

Table 2.3: Government Finance Variables Discontinuity Estimates

	Dependent Variable		
	Ln(FPM Transfers)	Ln(General Receipts)	Ln (General Expenditure)
Above the Cutoff	0.142	0.076	0.066
	(0.026)***	(0.019)***	(0.019)***
Observations	4,842	4,842	4,842
R-squared	0.413	0.663	0.634

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.4: Employment Discontinuity Estimates, Overall and by Education

Dependent Variable: Number of jobs					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	130.8	110.31	14.13	3.081	3.778
	(50.5)***	(40.52)***	(12.66)	(2.644)	(4.025)
Observations	4,842	4,841	4,841	4,432	4,696
R-squared	0.611	0.547	0.569	0.354	0.377

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.5: Log Employment Discontinuity Estimates by Education

Dependent Variable: Log(Number of jobs)				
	Worker's Education			
	Less than HS	HS grad	Some college	College grad
Above the Cutoff	0.154	-0.004	0.022	0.061
	(0.072)**	(0.065)	(0.089)	(0.087)
Observations	4,841	4,841	4,432	4,696
R-squared	0.646	0.576	0.480	0.538

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.6: Employment Discontinuity Estimates by Sector

Dependent Variable: Number of jobs			
	Sector		
	Private	Public	Other
Above the Cutoff	111.1	11.27	8.45
	(43.4)**	(14.27)	(13.62)
Observations	4,842	4,842	4,842
R-squared	0.557	0.258	0.416

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.7: Wage Discontinuity Estimates, Overall and by Education

Dependent Variable: Log Average Wage					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	0.006	-0.005	0.047	0.055	0.033
	(0.020)	(0.019)	(0.024)*	(0.046)	(0.042)
Observations	4,842	4,841	4,841	4,416	4,690
R-squared	0.456	0.541	0.357	0.193	0.247

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing wage information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.8: Wage Discontinuity Estimates by Sector

Dependent Variable: Log Average Wage			
	Sector		
	Private	Public	Other
Above the Cutoff	-0.019	-0.011	0.001
	(0.025)	(0.033)	(0.022)
Observations	4,789	4,832	4,529
R-squared	0.396	0.324	0.410

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing wage information for particular sectors in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.9: Two Stage Least Square Estimations

	Dependent Variable	
	Number of Jobs	Log Average Wage
Ln Gen. Expenditure	1,968.7	0.098
	(763.3)***	(0.294)
Observations	4,842	4,842
R-squared	0.551	0.479
First Stage F Stat	12.32	

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.10: Type of Government Spending Estimations

	Dependent Variable		
	Ln(Personnel Spending)	Ln(Investment)	Ln(Other Spending)
Above the Cutoff	0.063	0.137	0.050
	(0.023)***	(0.060)**	(0.025)**
Observations	4,839	4,820	4,841
R-squared	0.654	0.276	0.385

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing spending information in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.11: Discontinuity at Other Sources of Revenues

	Dependent Variable	
	Ln(Proprietary Revenues and Taxes)	Ln(Other Transfers)
Above the Cutoff	0.037	-0.003
	(0.056)	(0.031)
Observations	4,840	4,842
R-squared	0.651	0.542

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Difference in the number of observations across columns is due to missing tax revenue information in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.12: Sensitivity Test to Bandwidth Selection

Dependent Variable	Number of jobs		Log Average Wage	
Bandwidth	Coefficent Above	Std. Error	Coefficent Above	Std. Error
250	270.0***	85.1	0.014	0.035
500	206.0***	63.7	-0.008	0.028
750	126.0**	55.4	-0.000	0.023
1000	130.8***	50.5	0.006	0.020
1250	93.1*	48.3	0.019	0.018
1500	101.5**	46.1	0.015	0.017

Note: Each coefficient represent the impact of being above the first three cutoffs on each dependent variable. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.13: Employment Discontinuity Estimates, Optimal Bandwidth from Calonico et al. (2014)

Dependent Variable: Number of jobs					
	Overall	Worker's Education			
		Less than HS	HS grad	Some college	College grad
Above the Cutoff	240.93	166.82	44.975	9.341	13.087
	(137.03)*	(98.69)*	(35.255)	(6.046)	(8.907)
Bandwidth	332.2	334.3	375.0	380.8	322.2
Effective Observations	1,675	1,684	1,886	1,740	1,589

Note: Above the Cutoff indicates municipalities with population above the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff. Sample is restricted to municipalities in the years 2002-2007. Difference in the number of observations across columns is due to missing employment information for particular education levels in some municipalities. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.14: Placebo Cutoffs

Depedent Variable: Number of Jobs			
	Placebo Cutoff		
	11,887	15,283	20,377
Above the Placebo Cutoff	-106.9	-67.6	-187.2
	(72.7)	(86.2)	(127.8)
Observations	1,808	1,384	988
R-squared	0.586	0.628	0.671

Note: Above the Placebo Cutoff indicates municipalities with population above the placebo cutoff. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the relevant cutoff in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.15: Municipal and State-Year Fixed Effects

	Dependent Variable			
	Number of Jobs	Log Average Wage	Number of Jobs	Log Average Wage
Ln Gen. Expenditure	1,747.2	0.137	1,003.0	-0.135
	(737.7)**	(0.294)	(469.3)**	(0.298)
Observations	4,842	4,842	4,717	4,717
R-squared	0.594	0.501	0.136	0.251
First Stage F Stat	11.98		21.97	
Municipal Fixed Effects	No		Yes	
State & Year Fixed Effects	Yes		No	

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population the first three FPM cutoffs. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the years 2002-2007. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.16: All Cutoffs- Reduced Form

Dependent variable:	Ln (General Expenditure)	Number of jobs
Pooled cutoffs 1-3		
I[X > 0]	0.066	130.8
	(0.019)***	(50.5)***
Observations	4,842	4,842
R-squared	0.634	0.611
Pooled cutoffs 1-8		
I[X > 0]	0.058	191.4
	(0.018)***	(96.7)**
Observations	6,477	6,477
R-squared	0.795	0.666

Note: Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,500 inhabitants above and below each cutoffs. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

Table 2.17: All Cutoffs-Two Stage Least Square Estimations

Dependent Variable: Number of Jobs		
Sample	Pooled cutoffs 1-3	Pooled cutoffs 1-8
Ln Gen. Expenditure	1,968.70	3,297.4
	(763.3)***	(1,531.5)**
Observations	4,842	6,477
R-squared	0.551	0.644
First Stage F Stat	12.32	10.63

Note: First Stage: Ln Gen Expenditure is instrumented by indicator whether municipality has population above the first three FPM cutoffs in the first model and above the first eight FPM cutoffs in the second model. Controls: Quadratic polynomial of population from cutoff, allowing the coefficients of the polynomial to be different above and below the cutoff, state and year fixed effects, and municipality characteristics measured in the 2000 Census (share of employment in the informal sector, agriculture and median household income). Sample is restricted to municipalities with population with 1,000 inhabitants above and below the first three FPM cutoffs in the first model and above and below the first eight FPM cutoffs in the second model. Robust standard errors clustered at municipality level in parentheses.***<0.01, **<0.05, *<0.10.

CHAPTER 3

Extending the "Social Safety Net": Female Labor Supply and Pension Eligibility

What capacity exists for social insurance programs to *increase* labor supply? A 1991 legal change extended the coverage of pensions in rural Brazil to include large numbers of previously uncovered women, conditional on work requirements. This paper analyzes the extent to which women increased their labor supply in order to establish future pension eligibility. Using a differences-in-differences approach, I document two sets of empirical results: First, I find evidence that rural women did increase their labor supply to qualify for public pensions. Second, I find that the response differs substantially by cohort: rural women made immediately eligible by age temporarily increased labor supply, whereas cohorts of younger rural women also increased labor supply, presumably as an anticipatory response. Interestingly, these younger cohorts would not become eligible for multiple decades. These results shed light on the capacity of elderly workers to respond to financial incentives for old-age labor supply participation, in addition to the extent to which younger workers might be forward-looking in their responses to retirement incentives.

3.1 Introduction

The extent to which social security programs cover those in need has been a contentious public-policy issue. Those in favor of reducing "entitlements" often favor cutting benefits if a system is believed to be insolvent or if recipients are to be induced to work. Others in favor expanding coverage argue that it is not only an obligation, but also in the best interest of a government attempting to maximize efficiency. At the crux of the debate lies the nature of a response – how do workers react to an extension of the social safety net? What is most often hotly-debated is the response of workers who are most immediately eligible for benefits, and what is relatively overlooked is how workers who may be eligible in the non-immediate future may react. It is not clear *a priori* what the response may be, given the numerous factors that influence the decision to work. It is especially unclear how conditional benefits not available for several decades would affect an individual's current propensity to work as planning horizons and discounting are of some debate in the literature.

In addition to these broad questions about the relationship between non-wage compensation and the propensity to work, other issues about what can be expected of general policy interventions are also of interest. If some groups, such as women, have more in the wage of margins for adjustment, there may be a concentrated effect among them where non is specifically intended. If general policies may be increasing labor force participation much more drastically among some groups than others, then such findings are critical for future policy planning and execution.

This paper sheds light on these two issues by way of exploring a public policy change in Brazil that directly affected Brazilian rural households. In 1991, legislation was passed that lifted the pre-existing restriction of the allowance of only one public pension per household, available only to the household head. As rural household heads were overwhelmingly male, this policy change opened the door to many rural women becoming immediately eligible for pensions, although the change itself was not purported to target female workers. For recipients in rural households, the size of these pensions was especially large, and therefore such a change resulted in many households receiving sizable increases in pension wealth. The period around 1991 was accompanied by sizable increases in female employment. This paper seeks to examine the extent to which there might have existed a causal relationship between these two phenomena. Given that work requirements, and specifically work requirements that showed a *history* of work, were necessary in order for women to receive the pension, such a relationship seems to be, at least, *prima facie* plausible.

In order to evaluate this hypothesis, we turn to a difference-in-differences estimation strategy. We document two main findings. First, our estimates are consistent with the extension of pension eligibility increasing labor supply among females of all ages, through the channel of work requirements for said eligibility, as increased pension receipts actually *lag* increased working rates. Secondly, the results indicate substantial heterogeneity in the response when examining pension recipients by cohort: we find that labor supply increases were more temporary for older cohorts, and we also find that the labor supply increases were more permanent/sustained for younger cohorts. Given that there was no retirement or earnings test for the pension, this finding is in line with the notion that female workers (re-)entered the workforce solely to qualify for the pension and that younger cohorts increased their participation in an effort to qualify for a future pension.

Existing Studies

There exists ample discussion in the academic literature that of how eligibility requirements for social benefits, and in particular, for social security might affect economic choice variables; however, much of this literature has focused on contexts in developed countries. For instance, Powers (1998) and Neumark and Powers (1998) address how means-testing as a feature of US Social security affects the incentives and resultant saving of American workers. Neumark and Powers (2000, 2004) expand on this work by examining how workers react in terms of labor supply, and find that they do, in fact, labor supply. Other work has demonstrated that younger workers can react in a forward-looking manner to eligibility criteria. Coile and Gruber (2007) finds that workers are forward-looking in their retirement decisions. The consensus from these studies seems to be that older workers are responsive to social security programs: they can, and do, reduce savings, and most relevantly for this study, labor supply in response to the incentive schemes posed by social security eligibility criteria.

Studies with similar research countries in the developing world have been much more scant. Though "workfare" programs in the developing world such as NREGA have been found to increase employment (Imbert and Papp (2012)), links between the stipulations of social insurance programs and their effects on employment are much more difficult to come across. With an eye toward old-age social insurance, Ranchhod (2006) finds that workers do reduce of labor supply to qualify for the means-tested South African pension, indicating that workers are reactive – however, the extent to which they are willing and able to adjust their labor supply *upward* still seems to be an open question. In this context, my paper explores the extent to which the *expansion* of social insurance programs might lead workers, and particularly female workers with margins along which to respond, to *increase* their labor supply in an attempt to qualify for conditional benefits. While "workfare" and other social programs are often formulated with the intention of increasing individual labor supply, social insurance programs, let alone *pensions*, are rarely thought of as an effective policy tool.

The pension reform under consideration in this paper has also been the subject of study. Three studies in particular use the social security reform we examine as an identifying source of variation. de Carvalho Filho (2008, 2012) estimate, respectively, how eligibility for social security benefits reduces the propensity to work and how increases in income reduce the incidence of child labor. Additionally, Ponczek (2011) finds that the pension had positive effects on child education. These studies find that increases in income brought about by the expansion of the program seem to improve outcomes for households by increasing various forms of consumption and providing capacity for investments in the future. Though similar to these studies in nature, our work seeks to identify the effects of a particular *aspect* of the reform, rather than the general income effect: the expansion of the pension program to cover multiple members within the same household, and specifically, previously ineligible females.

As far as we are aware, no study has focused on the eligibility effects of the pension program. The results we find suggest that older workers, and those who are often considered beyond their working years, can actually increase their labor supply when given the incentives to do so: we find a temporary increase in the probability of working among potentially eligible workers following the implementation of the program, presumably in response to the eligibility criteria set out by the change. Additionally, workers that will not be eligible for at least a decade seem to increase their labor supply, however, as opposed to the temporary nature of the labor supply increase of older workers, this labor supply increase seems to be somewhat permanent. Overall, our results indicate that changes to the eligibility criteria of social security programs can have tangible responses for several generations of workers, including those who may become eligible in the distant future.

The paper proceeds as follows. Section 3.2 outlines the data we use for estimation as well as the background of the policy reform. Section 3.3 describes the identification and estimation strategy, and section 3.4 provides the empirical results. Section 3.5 discusses potential threats to identification. Section 3.6 provides a discussion of the results in the context of existing economic models, and section 3.7 concludes.

3.2 Background

3.2.1 The 1988 Constitutional Reform

Brazil has long kept separate social security regimes for rural and urban workers. In 1971, when the country was still under military rule, a formal rural pension scheme (PRORURAL) was established, allowing certain benefits to rural workers and their households. Specifically, under this regime, rural workers were allowed an old age benefit of 50% of the prevailing minimum wage upon turning age 65. However, only the eligible member of a household was the head, who was overwhelmingly likely to be male. For reasons that will become clearer later in the paper, it is important to note that this and future retirement benefits (henceforth, "pensions") were not means or retirement tested.

As the country moved toward democratization in the 1980s, first informally then officially, a new constitution was established in 1988 that, among other things, sought an equivalence of benefits and services for both urban and rural populations. The constitutional precepts were accompanied by a supplementary law enacted in 1991 that provided more specifics on how such equalization was to take place.

As part of the passage and the subsequent enactment of Law (*Lei*) #8212/8213 in 1991, benefits were expanded in scope and in magnitude. Whereas the previous minimum age to qualify for a pension was 65 for both rural male and female workers, the new law stipulated that rural males would become eligible at age 60 and rural females at age 55. The qualifications for pension eligibility included an economic activity in addition to age, which will be discussed later in this section. Additionally, the law set a lower bound for a pension payment equal to the minimum wage (rather than 50% of it). Furthermore, and most importantly, the law allowed for households to receive multiple pensions, which allowed for female spouses to begin receiving social security benefits in addition to their husbands.

This last change represents the relevant variation for this study. We compare the labor supply decisions of females made eligible (and potentially eligible) under different policy regimes. We find evidence that the policy did change the labor supply decisions of older females, and the temporary nature of the response and subsequent dropout leads us to conclude that these females were participating in the economy strictly to qualify for the pension. However, not all of the induced activity was short lived: we find other evidence that *younger* females increased and sustained participation in expectation of future eligibility.

Previous work from de Carvalho Filho (2008) shows evidence that the increase in the number of rural pensions did not actually increase for a few years following the enactment of the 1991 legislation; however, by the fall of 1993, most of the increase in pension take-up had occurred. As a confirmation of this phenomenon, we show in figure 3.1 are the numbers of pensions granted over the time period 1981 to 1997, the years surrounding the change in the pension policy. A sharp increase in the number of rural pensions seems to be evident from 1992 onward, consistent with the notion that there was some delay in either informational uptake or administrative delivery of the newly legal pensions (urban pension grants are not expected to increase over this period, as the eligibility criteria were not changed to the same extent).

Pension Eligibility

As aforementioned, Law 8212/8213 made explicit as possible the stipulations regarding pension eligibility. In addition to age, pension receipt also required "discontinuous" proof of economic activity for a specified amount of time. "Discontinuous" refers to the idea that the proof did not have to be shown for a continuous period that counted toward eligibility; it just had to be shown at point(s) during the period, making for a seemingly subjective and informal requirement. What counted as proof was also subjective, with pay stubs and similar documents counting, but also with signed affidavits from government officials. Finally, qualification for rural work could also be left to the judgment of the pension office.

The period for which an applicant was required to show activity was increasing over time. Those who applied at the time of the policy were required to have had activity for 5 years (albeit "discontinuously"), and those who applied later were required to have activity for a longer period of time. The concept in mind was to have workers eventually reach 15 years of activity before qualifying for a pension, and at the time of the policy, a grace period was put in place to allow for adjustment. Table 3.1 shows the increasing activity time as outlined during this grace period. Importantly, younger generations (i.e. those who applied later) were expected to have longer periods of activity before becoming eligible.

Overall, it seems as though work requirements were nominal based on what was codified, however given the specificity of what was required, it would seem that the requirements were enforced, at least in some areas. However confusing the requirements were to workers, a *sufficient* means by which to qualify for the work requirement was to actually participate in economic activity.

Pension Value

What was the actual value of these pensions? As aforementioned, the change stipulates that the minimum value of rural pensions should be equal to the minimum wage. Figure 3.2 plots the aggregate number of pensions granted against the value of the nominal minimum wage.[1] For unknown reasons, the per-person amount granted was actually *less* than the nominal value of the minimum wage in over several years, however as administrative uptake was purportedly slow with the disbursement of the pensions, it was likely the case that errors in administration led to the pensions being granted on amounts based on the previous regime (which had no established floor). Additionally, income data for the sample of our study is scant, but given that the minimum wage *is* the floor, it seems likely that most workers in the sample earn no more than the minimum.

Unfortunately, calculating the exact value of the pensions received as part of the old-age benefit program is problematic, as the only pension variable in our dataset also includes any benefits received as part of the survivor's benefit and the disability program. However, as a fraction of total household income, pensions comprise a significant portion – in 1992, a single minimum wage (the value of a pension) was roughly 45% of the average rural household income, and over 50% of the median. In 1999, due to rising rural incomes, the minimum wage was roughly 30% of the average rural household income, and roughly 45% of the median. The pensions disbursed as a result of this reform were therefore a potentially large

[1] Unfortunately, aggregate data on pensions granted and their values prior to 1994 is unavailable. This may not be as surprising considering that over this time a new currency regime was being implemented.

part of the household budget.

3.2.2 PNAD

The *Pesquisa Nacional por Amostra de Domicílios*, or PNAD, is annual, nationally representative survey with an emphasis on labor market activity given to roughly 100,000 households in Brazil. The survey asks detailed questions about the nature of work and demographic aspects of the household, and importantly for this study, it contains information on pension receipts by those in the household. In creating our analytical sample, we use data from years surrounding the 1988 legislation and the 1991 reform. Specifically, we use data starting from 1981, which includes data from the years 1981, 1982, 1984-1990, 1992, 1993, and 1995-2001. Years over this time period that were omitted are years in which there was no PNAD conducted.

The PNAD underwent significant changes following a census conducted in 1991. Among these changes, two are particularly relevant. First, the employment part of the PNAD questionnaire changed, expanding what was previously considered employment and including domestic work as economic activity. Importantly, the order and the way in which it asked what a respondent was doing in the reference week changed. The PNAD from 1992 and onward changed in such a way as to expand the definition of employment to include work, particularly work that may not have been considered market labor. As such, the specific question used in my analysis changed in format. Prior to 1992, the questionnaire (shown in Figure 3.16) asked in what activity the respondent had been engaged during the survey's reference week, of which "work" was an option. Other options included "had job but did not work", "looked for a job", " was a student", "performed household chores", "retired or pensioner", and "other." In the PNAD of 1992 and onward (shown in Figure 3.17), the questionnaire asked questions regarding activity in the reference week in a different format. First, the respondent answered if he/she had "worked" in the reference week. Next, the respondent was asked subsequent questions about engagement in either agricultural or animal cultivation for family provision and about engagement in any kind of construction on the home unit. Finally, the respondent was asked whether he/she was temporarily removed from paid work. If the respondent answered positively to any of these questions, he/she was considered employed.

To circumvent issues surrounding this change, I use as my main employment outcome an indicator for whether the individual reported "working" in the reference week, that is selecting the first option prior to 1991 and selecting "yes" as the answer to the first employment question after 1991. While these two questions should capture the same response, there may be some understandable doubts as to whether anything else cofounding my results might be occurring as a result of this change. To mitigate these fears, I also focus also on employment groups that should not be affected – namely the propensity of individuals to be working in occupations that existed in both pre and post periods, and the propensity of individuals to be working in agriculture [2].

The second issue involves how "rural" and "urban" were determined before and after the 1991 census. Because the boundaries used to determine urban and rural areas change depending on the laws in use by municipalities, the IBGE sets the boundaries of urban and rural areas based on the last census. The only census in our sample was taken in 1991, which coincides with the enactment of the policy in question. Thus, the boundaries of the locations of rural and urban areas changed at a time coinciding with the policy change. However, it should be noted that that when examining the difference across other census years (2000 and 2010), there seems to be no suspicious "jump." [3] [4] This finding seems to confirm that the change did not seem to affect the would-be experiment in other census years. Additionally, "rural" status can be precisely defined using job occupation codes, and therefore our use of geographic coding simply works as a proxy for the specific definition. However, past work (for instance de Carvalho Filho (2012)) finds that the use of this *same* geographic proxy over the *same* change in rural specification does not matter in the determination of results. Therefore, we stick with this geographic proxy for simplicity and because we do not feel as though the changes pose a concern. [5]

3.2.3 Analytical Sample

Table 3.2 gives a snapshot of the sample used for the main analysis, and Table 3.3 gives a snapshot of the rural women in the sample. The majority of the sample is urban women, and working rates within the sample are relatively low, standing at an average of about 36%. Most women have a male present in the household, and a very small minority of women live alone. Moreover, the women in our sample live in relatively large households – on average, household size is over 5 individuals.

The sample used for the estimation of our main results is quite large, with roughly 2.5 million observations. However, in supporting analysis, this sample shrinks as we focus on certain interest groups – namely individuals who report working in certain types of jobs. More will be explained as that analysis

[2]The change in the survey was also accompanied by the addition of several job occupation codes that were not available for selection prior to 1991.

[3]This analysis is available on request.

[4]A "rural" worker in Brazil is defined, somewhat ambiguously, by having a job in the "rural" sector. Hence, while technically a worker's occupation should determine his or her eligibility for a rural pension, geographic location is arguably a better predictor than an occupational code.

[5]Additional analysis was conducted defining treatment and control groups in terms of whether a male present in the household was working or had worked in the agricultural sector. Using these definitions resulted in largely the same qualitative results. This analysis is available upon request.

is discussed.

3.3 Identification and Estimation

The following equation serves as the primary equation of estimation for the identification strategy outlined in this paper.

$$y_{ist} = \gamma_s + \mu_t + \pi D_i + \sum_{j=0}^{J} + \beta_{1991-j} D_{i,s,t-j} + \sum_{k=1}^{K} \beta_{1991+k} D_{i,s,t+k} + X'_{ist}\delta + \varepsilon_{ist} \qquad (3.1)$$

specifies the relationship between an outcome y_{ist} and several explanatory variables, in which γ_s represents state-fixed effects, μ_t represents year-fixed effects, D_i is an indicator for individual i's"treatment" status (in our case, rural household member), of state s in year t. The indices j and k represent the number of pre/post years, respectively, of the study, and X_{ist} represents a vector of individual-household characteristics that are thought to be correlated with the outcome [6].

This specification is similar to that of Autor (2003). In this context, the coefficients of interest in our estimation procedure are the k β_{1991+k}'s, which represent the treatment effect in different periods following the legal change of 1991, relative to an omitted year. In our estimation, we omit 1981: hence, our coefficients can be thought of as the difference in outcomes between rural and urban women, relative to a baseline difference measured in 1981. The model is equivalent to an "extended" difference-in-difference specification in which the treatment effect can be measured over time (and in fact, in each period), as opposed to a more general specification in which a post-period average treatment effect is estimated. Such a model is particularly useful for estimating potential short-lived responses.

The identifying variation in this model (providing us with k unbiased estimates of the β_{1991+k}'s) comes from the assumption that treatment status is independent of the policy-change that affects the treated group differentially. In our study, this identifying assumption entails that both rural and urban women's outcomes were moving in parallel prior to the enactment of the 1991 policy[7]. It also entails that nothing else happened that led to differential outcomes between treatment and control (rural and urban) groups in the same time period.

[6] These include the number of other pensions received by members of the household, the presence of a male member, and the size of the household

[7] Coefficient estimates of the treatment effect (i.e. the difference) prior to 1991 are statistically zero, as figure 3.8 shows. While this is not a sufficient condition for the parallel trends assumption, it is a necessary one.

Errors are clustered at the sector-state level (i.e. each combination of sector and state). This is due to the assumption that unobserved error terms may be correlated within the same sector and state, but not across.

3.4 Results

This section reviews results from the empirical difference-in-differences analysis. Initially, we discuss the estimated results across all cohorts of females. Next, we explore the notion that the effect was temporary by discussing the response among the first initially-eligible cohort of females. Finally, we examine the response among younger and older cohorts of women.

As aforementioned, my analysis centers on the construction of an employment variable that indicates only if the respondent worked in the reference week, i.e. marked the first option to the question prior to 1992 or answered positively to the first question from 1992 onward. While these questions both ask about essentially same thing, a concern is if positive response rates are higher from 1992 onward, confounding the finding of higher employment post-policy. One issue is that the two questionnaires present different alternatives to the question of having "worked" in the reference week – if the presentation of different alternatives results in a higher response of "working" in the post-policy period, then our results may be inseparable from the effect.

While the effect of the survey change is unfortunately collinear with my purported treatment, I appeal to the heterogeneity in the responses among different age groups to argue that the survey change cannot account for my findings. Firstly, as will be seen later, certain cohorts had *temporary* increases in employment consistent with an attempt to work in order to qualify; the change in the survey cannot account for such a response as the nature of the survey changed only once during my sample period. Secondly, as will also be seen, the response among older women was substantially *larger* than the response among younger women. If a change in the survey accounted for the result we see, we would expect the change to affect groups equally. Given these points, it should be noted that the survey change cannot be ultimately responsible for my findings.

3.4.1 Main Results - All Females

The graphical plot of the outcomes for both rural and urban women is shown in Figure 3.4. (As can be seen, there seems to be a clear divergence in pubic pension receipts for rural women as opposed to their urban counterparts in the top figure. In the bottom figure, there also seems to be a divergence in working outcomes. Prior to 1991, propensity to work among rural females was lower than it was among urban females, however, this difference essentially became negative as working propensity among rural females overtakes that of urban females in the years immediately following the change.

Table 3.4 shows the point estimates of the differential treatment effect by year, relative to the difference between rural and urban women in 1981. These coefficients are plotted with 95% confidence intervals in Figure 3.8. In the years immediately following the legal change, the difference in working propensity was about 6-7 percentage points higher than it was between 1982-1987, relative to the baseline year of 1981. The effect seems to dissipate in following years.

It is important note that that the change in the propensity to work *preceeded* the change in the receipt of government pensions – the change in the difference in government pensions did not start to take off until 1993, lagging the initial change in working propensity. This observation is our first clue that rather than pensions *increasing* work supply (implying an awkward negative income effect), the increase in propensity to work was instead *anticipatory*. More evidence for this will follow in the next sections. Additionally, it is important to note that the coefficients of the effects on the outcomes are *larger* than the coefficients of the effects on the pension-receipt variables – this result hints at the notion that the pension policy could have affected non-recipients, which will be explored in the next few sections.

Overall, it appears from these general results that policy change increased labor supply, at least on the extensive margin, among rural women as opposed to their urban, ineligible counterparts. Whether this effect was driven by a temporary increase from women seeking to work just enough to qualify, or by a more permanent shift in behavior will be shortly discussed as we move into analysis of specific cohorts.

3.4.2 Analysis by Cohort

Analysis of Immediately-Eligible Cohort

In order to determine the factors that surrounded immediate eligibility, we turn to the cohort of women aged 55-64 in 1991, at the time of the passage of Law 8213. These women are those who would have become immediately eligible under the law change. We track the outcomes for the urban and rural cohort of these women, and the trend in the top row seems to be clear – the initial cohort of immediately eligible women increased their working propensity, which quickly fell again as the pensions was being disbursed, and again, the increase in working propensity leads the receipt of pensions. It seems as though this cohort of women was working *specifically* to obtain the pension, and dropping out after receipt. [8] The plotted coefficients from Table 3.5 in Figure 3.9 indicate a significant (and substantial) 19 percentage point increase in the difference between rural and urban members of the immediately eligible cohort, which again drops to insignificance in later years.

Within-Rural Comparisons: Newly and Formerly Eligible Women

We also examine how this initially eligible cohort of rural women (aged 55-64 in 1991) compares in outcomes to other, ineligible rural women. Given that though younger women may not have met the age qualification at the time of implementation, they were more likely than not *eventually* eligible, and to the extent there may be a response among such individuals we remove them from the analysis. Our "control" group in this within-rural comparison is therefore women whose eligibility for a pension did not change as a result of the policy: that is, women above the age of 65 (and below the age of 90) who were heads of their households. This control group's eligibility status did not change as a result of the pension as they were previously eligible under the PRORURAL policies in effect prior to 1991.

As can be seen in Figure 3.5 and the associated adjusted differences plotted in Figure 3.10, there seems to be an apparent jump upward in working rates, which then dissipates, presumably as this cohort was qualifying for and obtaining the pension. These results serve to galvanize the notion that even when holding constant factors that would have affected all rural workers, the most immediately eligible cohort of women was induced to work, given the work requirements that qualifying for the pension required.

It does seem as though there is a slight uptick in working rates among the older, ineligible cohort. Upon closer inspection, this uptick seems to be driven by some of the younger members (i.e. those aged between 65-70) [9]. While we cannot say for certain what is driving the uptick, it may be attributable to some confusion as to the requirements of the program or elderly heads receiving pensions via another mechanism being alerted to their previous old-age pension eligibility by the program. It may also be the case that there is indeed a change common to all rural workers, as there does seem to be a slight uptick for men aged 65-85 also [10]. Whatever the case may be, our results still highlight a large uptick for the eligible group than the uptick among younger formerly eligible females.

[8] In ancillary analysis, we show using a regression-kink design that there did seem to be a differentially significant relationship between dropout rates and pension receipt before and after 1991. Again, this analysis is available upon request.

[9] This analysis is available upon request.

[10] Again, this analysis is available upon request.

Analysis of Younger Cohorts

In Figures 3.13, 3.14, and 3.15, a similar pattern emerges – an initial jump in the propensity to work followed by various degrees of permanence. Interestingly, it seems as though the younger the cohort, the more permanent the initial jump. This observation is consistent with younger cohorts anticipating that they will have to show evidence of work for a longer (albeit mysteriously "discontinuous") amount of time.

In order to shed light on this phenomenon, we look specifically at the cohort of rural women aged 35-44, treating this cohort as a "treatment" cohort and treating the urban cohort of women the same age as a control group. If expectations of future benefits play no role, there should be no discernible differences in the working behavior of these two groups over the sample period. However, if there is indeed some effect of the policy, we may expect an increase in working rates among those who *will be* affected directly following its implementation.

As can be seen in Figure 3.6 and the associated coefficient estimates in Figure 3.11, the predicted pattern for the younger cohorts seems to emerge. While the difference in pension uptake rates seems suspect, it should be noted that (1) the difference in uptake rates is quite small, but precisely estimated, given the size of these cohorts, and (2) that this difference in uptake rates does not seem to be driven by the 1991 legal reform as it moves in the opposite direction and occurs later, after the spike in pension flows [11]. However, the difference in working rates is large and significant, and it begins almost *immediately* after the reform. Future-eligible rural females increase their working rates across the policy years by around 10 percentage points to over 51% from a baseline of around 42%, whereas unaffected urban females hover around 54-55%.

A similar exercise can be done for the younger cohort aged 30-34 in 1991, and results for this cohort are perhaps more salient given that there does not seem to be any statistical difference in the urban/rural pension uptake gap, yet there does seem to be a differential difference in the working rates between urban and rural females [12]. Figure 3.7 contains a visual representation of the difference and Figure 3.12 shows the associated coefficient estimates.

[11] There is actually no statistical difference in the uptake rates for the 30-40 age group; however, I include these results for the sake of transparency.

[12] Note that this cohort, and not the cohort aged 25-34, was chosen due to the notion that low working rates in both groups during the pre-period, where both groups were teenagers, would have posed a problem for estimation.

3.5 Robustness of Empirical Results

3.5.1 Parallel Trends and Regression Specifications

Given the nature of the data used to estimate the results (a repeated cross-section), it is natural to wonder about the validity of the results. As in any difference in difference, our identification may be compromised if there were divergent trends in the treatment and control groups prior to the policy change (i.e., a violation of the "parallel trends" assumption). There may also be questions about the causal driver of our results if there were to be another policy overlapping with what we allege to be our identifying source of variation. While we cannot offer evidence in favor of the latter, upon visual inspection there appears to be no cause for alarm.

Additionally, the use of different regression specifications (i.e. the inclusion of year fixed effects rather than a single "post" dummy) to estimate our results does not seem to substantially change the point estimates. One fear is that a change in the composition of households may be driving our results. If, for instance, females that are part of the same family are splitting up to form new households in order to receive more pensions, we may expect an increase in working behavior if these households then have more dependents. However, controlling for household composition variables does not seem to change the significance or the magnitude of the point estimates.

3.6 Discussion

Even when restricting to this sample of agricultural workers (which represent some, perhaps a majority of rural work, but not all), I find even stronger results from the difference-in-difference estimation. Thus, even when restricting to within agricultural labor free of any employment classification changes, I find a substantially higher propensity to work among eligible (or future-eligible) workers. There seems to be a clear temporary response on the part of immediately eligible workers, and a more sustained response among the future-eligible workers.

The evidence seems to point to the notion that having a pension with an informal work requirement seemed to substantially increase labor supply among rural females. While the responses seem to be much larger than what has been documented in other contexts (or might seem sensible from another perspective), it is important to keep in mind how sizable pensions were. Given how important the equivalent of an additional minimum wage might have been to a lower-income rural family, it becomes less unreasonable to expect that the requirements had such large impacts.

Of course, our results rely on the truthfulness of reporting in the PNAD questionnaire. It is possible (though not necessarily plausible) that workers believed their answers to the PNAD were indicative of the working status that would have been used for their pension qualification procedure. However, in this case, it is unclear why there should be any sustained working habits among the younger cohorts, once the older cohorts had dropped out and had presumably passed on the information regarding what was required, or, for that matter, why there should be any response among the younger cohorts at all.

One may also wonder as to how much effective labor supply was increased, as our study focuses solely on the extensive margin of work. It is also possible that workers may have increased their labor supply on the extensive margin but engaged in relatively small adjustments on the intensive margin. However, it is also important to keep in mind that (i) the relevant outcome for pension qualification is extensive margin labor supply and (ii) a focus on "hours of work" may be a bit irrelevant as a substantial fraction of the rural sector is agricultural and not employed by "firms" in the traditional sense, (iii) our outcomes focus on individuals who have reported working a minimum of 15 hours per week; hence, the result we identify is not driven by individuals entering the workforce or selecting in due to a previously-existing hours restriction.

Notwithstanding these points, it remains to be explore how much of a potential productivity shock this increase in labor supply among the rural population provided. While the appropriate "check" on productivity would be difficult to pinpoint, this represents a promising area for future work.

3.7 Conclusion

This paper has shed light on the willingness and the ability of workers to react to retirement incentives in a forward looking manner. The results regarding the immediately- eligible cohort indicate that elderly workers still have the ability to increase their labor supply given the right incentives, and the results regarding the younger cohorts indicate that retirement policies enacted today may have unforeseen effects among those whom may not be eligible currently but will be in the future.

This study adds more broadly to the literature regarding retirement policies in the developing world. Reforms of benefits and social security often demand an analysis of the associated labor supply responses among the eligible cohort. However, this paper shows that an expansion of benefits, can, under some circumstances, *increase* labor supply if qualifications are properly managed. Work on the intensive margin of adjustment and on the aggregate product such changes can have remains to be completed.

3.8 Figures

Figure 3.1: Number of Pensions Granted by Year, 1980-1997

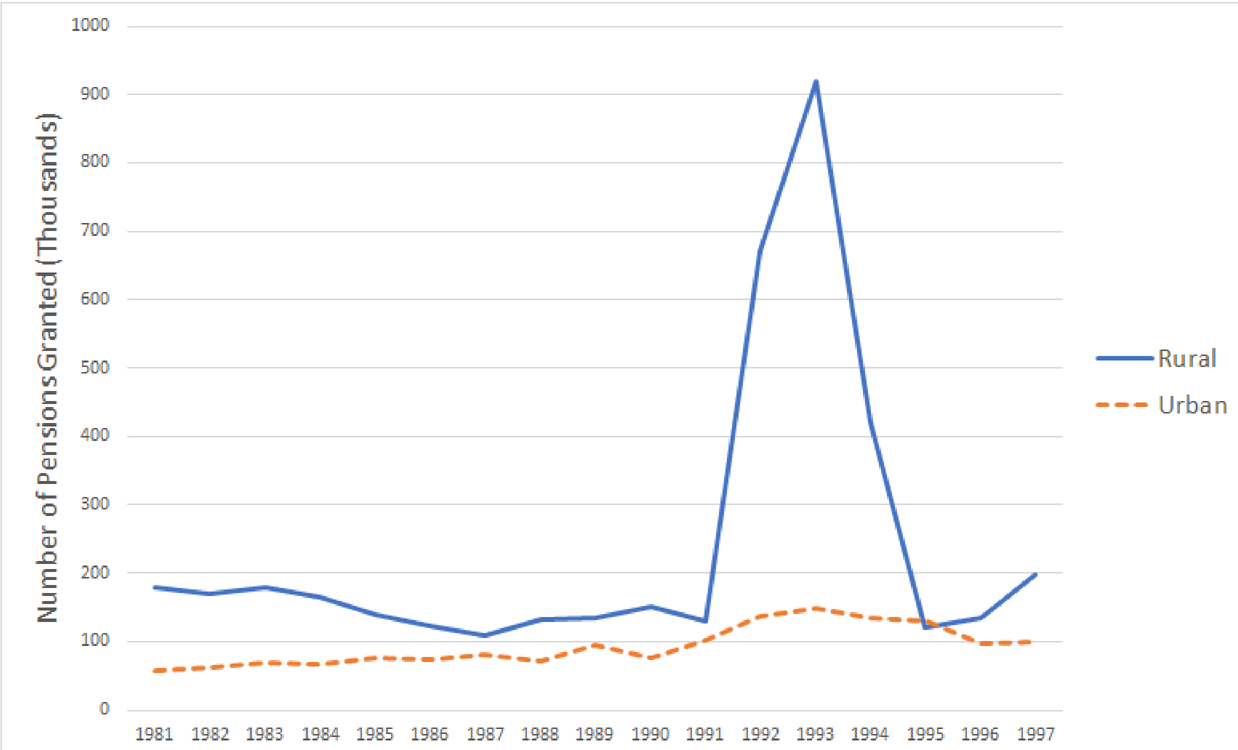
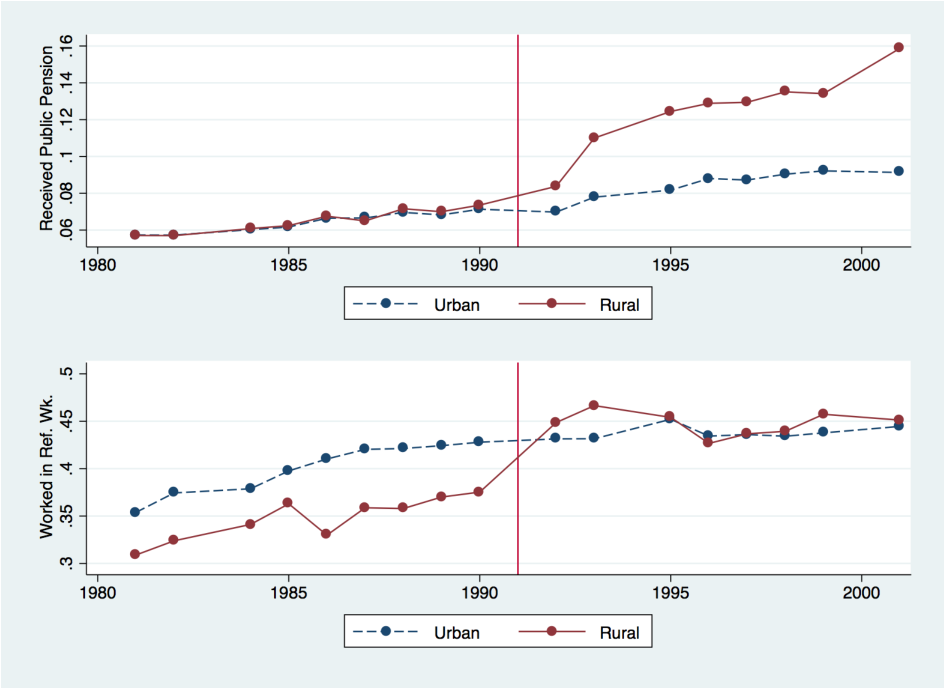


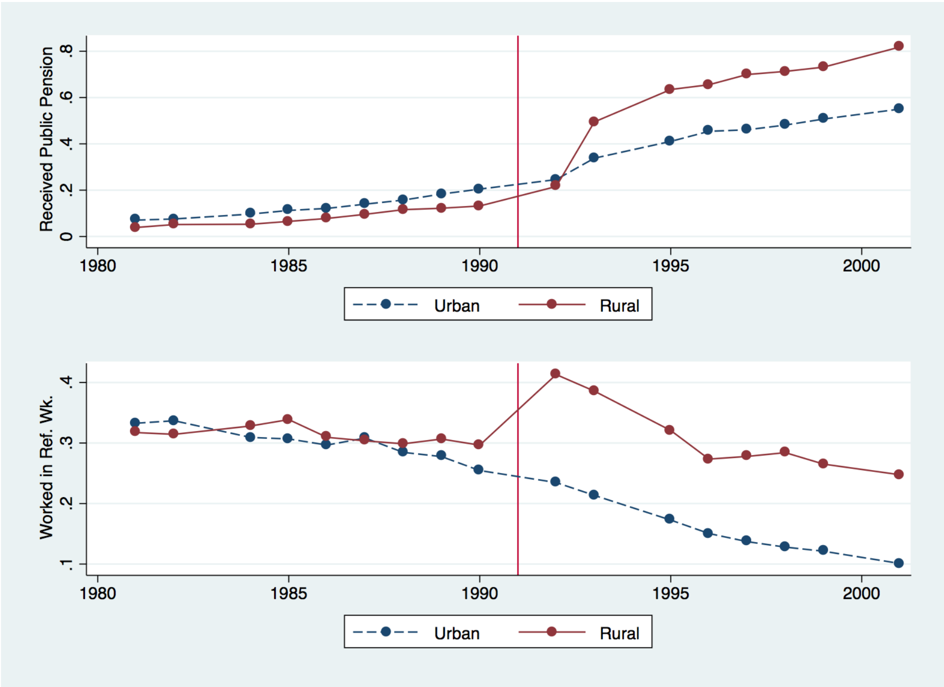
Figure 3.2: Per-Person Pension Amounts by Year, 1994-1997

Figure 3.3: Outcomes for All Females, by Urban/Rural



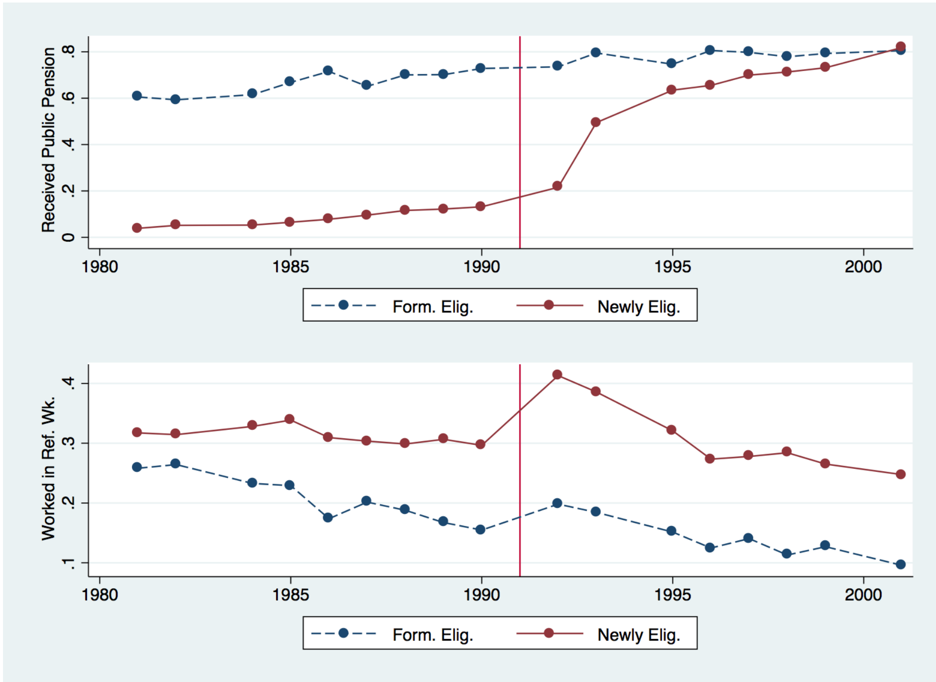
Note: Points represent unconditional age-group means. Estimates of the adjusted difference, accounting for fixed effects and covariates and relative to the difference in 1981, are shown below in Figure 3.8.

Figure 3.4: Outcomes for Females Aged 55-64 in 1991, by Urban/Rural



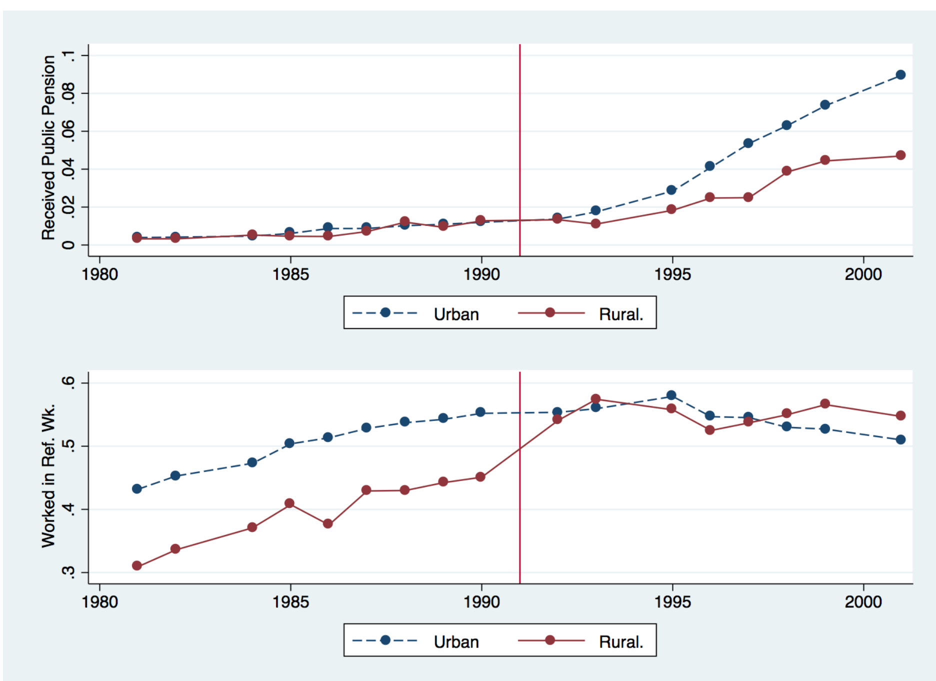
Note: Points represent unconditional age-group means. Estimates of the adjusted difference, accounting for fixed effects and covariates and relative to the difference in 1981, are shown below in Figure 3.9.

Figure 3.5: Outcomes for Newly Eligible and Formerly Eligible Rural Females



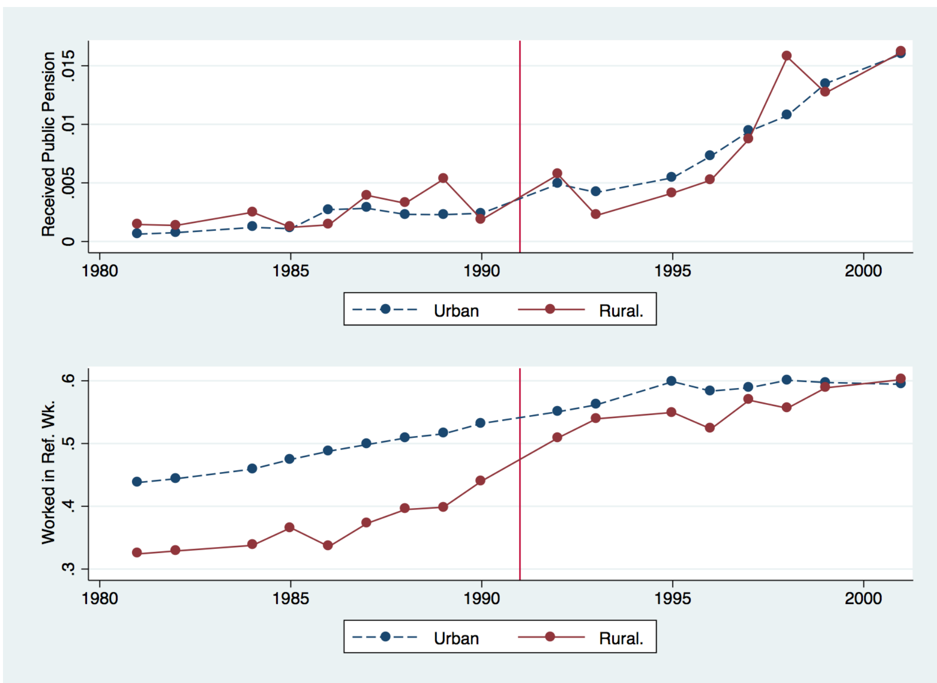
Note: Points represent unconditional age-group means. Estimates of the adjusted difference, accounting for fixed effects and covariates and relative to the difference in 1981, are shown below in Figure 3.10.

Figure 3.6: Outcomes for Future Eligible Females (Ages 35-44 in 1991)



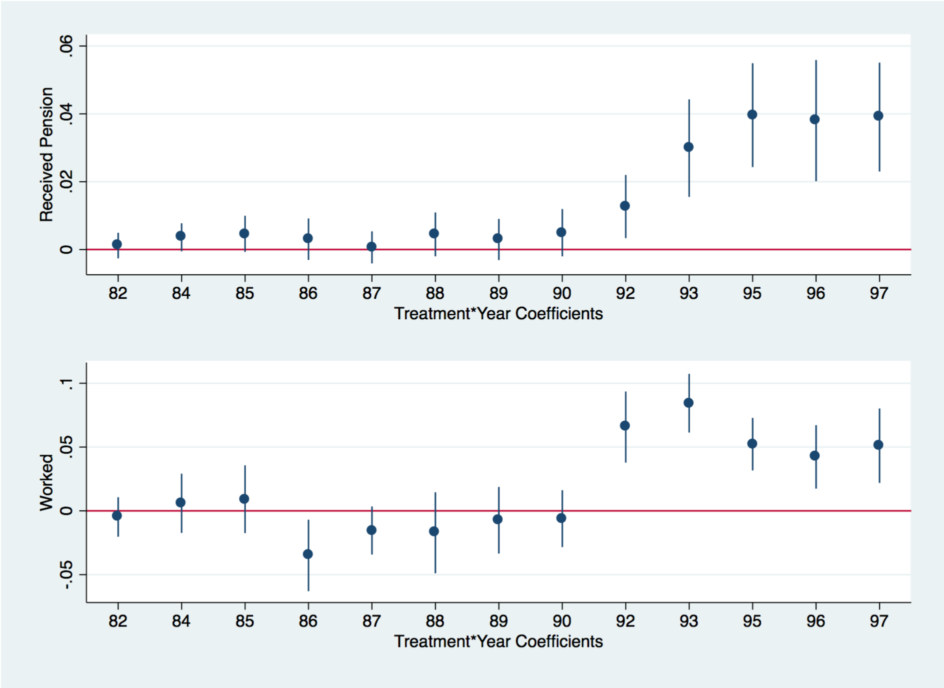
Note: Points represent unconditional age-group means. Estimates of the adjusted difference, accounting for fixed effects and covariates and relative to the difference in 1981, are shown below in Figure 3.11.

Figure 3.7: Outcomes for Future Eligible Females (Ages 30-34 in 1991)



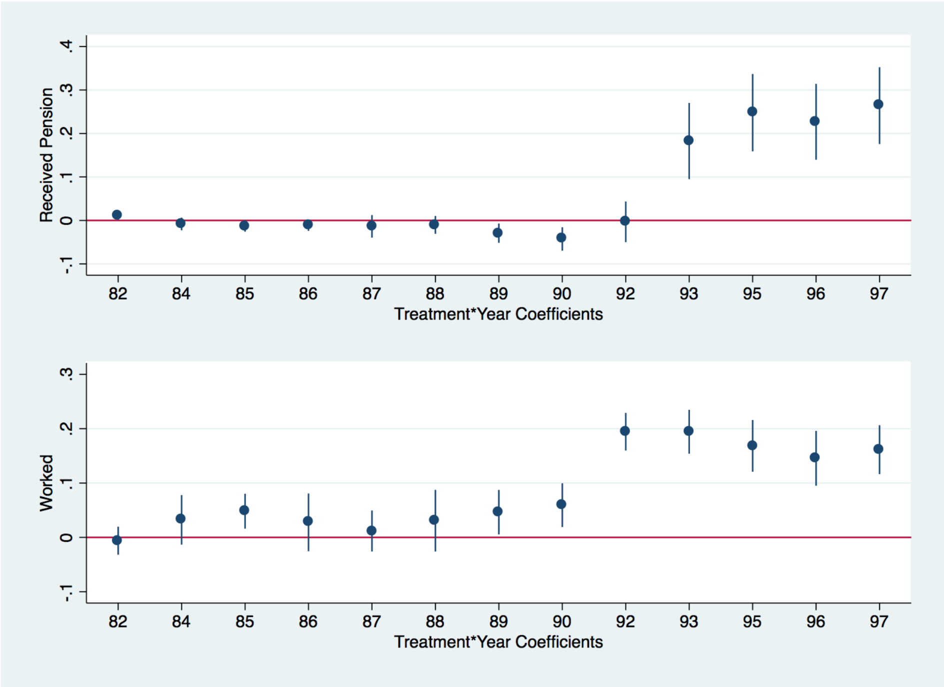
Note: Points represent unconditional age-group means. Estimates of the adjusted difference, accounting for fixed effects and covariates and relative to the difference in 1981, are shown below in Figure 3.12.

Figure 3.8: Difference-in-Difference Year Coefficient Estimates for All Rural Women



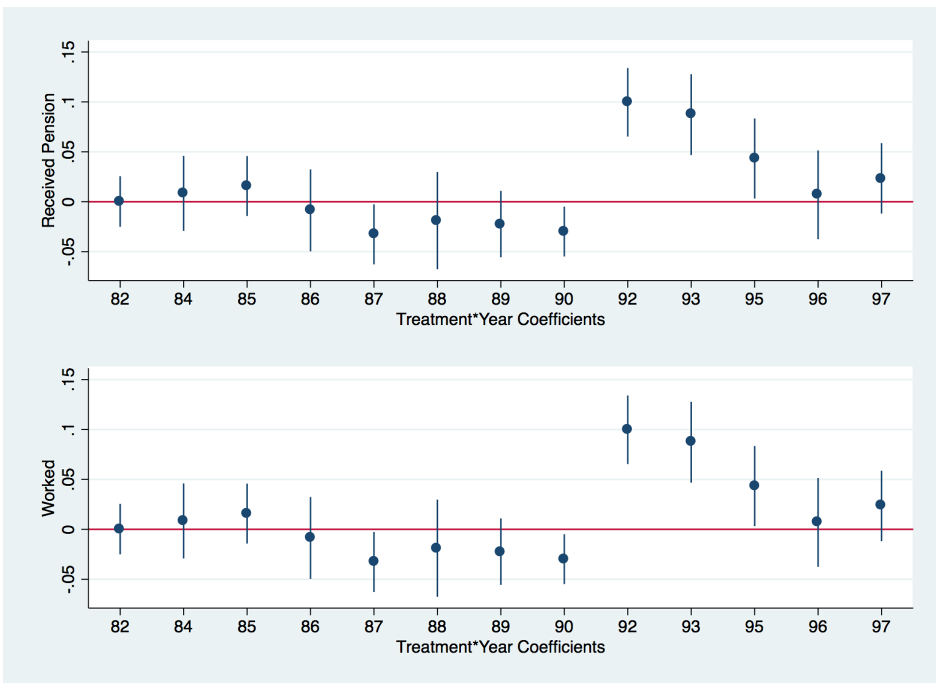
Note: Estimates are shown controlling for state and year fixed effects in addition to household composition variables (the number of additional pensions, an indicator for male presence, and the size of the household). Estimates reflect the treatment effect in the indicated year, relative to the baseline difference in 1981, the first available time period of data.

Figure 3.9: Difference-in-Difference Year Coefficient Estimates for Immediately Eligible Cohort (Ages 55-64 in 1991)



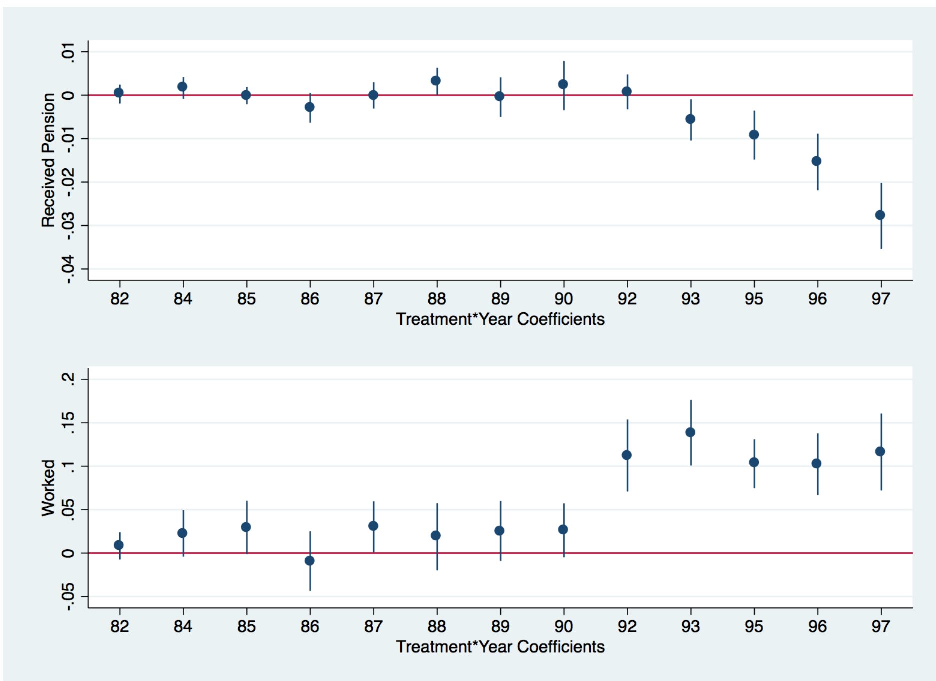
Note: Estimates are shown controlling for state and year fixed effects in addition to household composition variables (the number of additional pensions, an indicator for male presence, and the size of the household). Estimates reflect the treatment effect in the indicated year, relative to the baseline difference in 1981, the first available time period of data.

Figure 3.10: Difference-in-Difference Year Coefficient Estimates for Newly Eligible and Formerly Eligible Rural Females



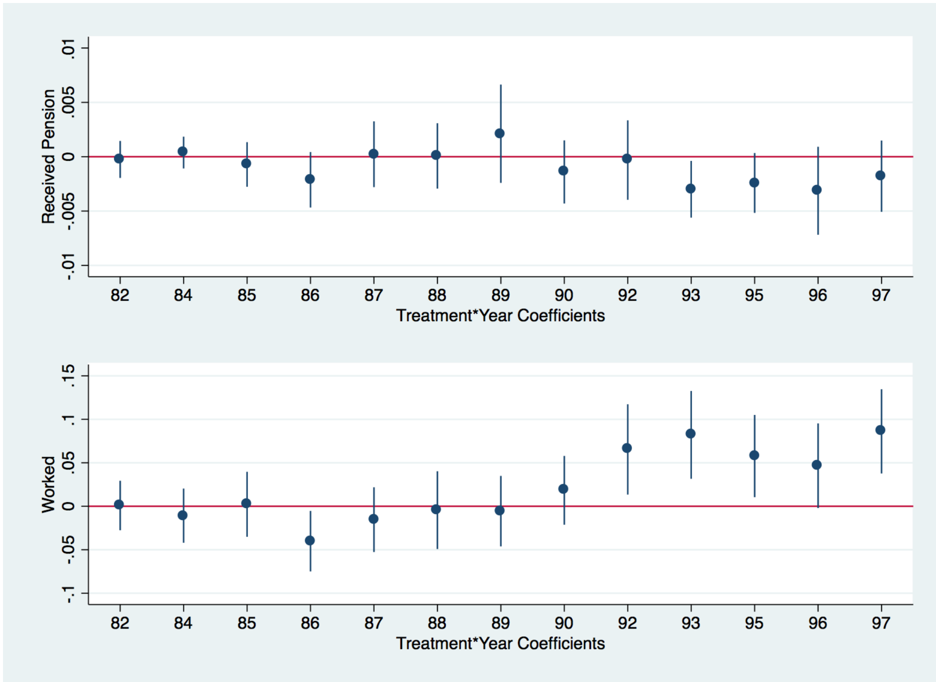
Note: Estimates are shown controlling for state and year fixed effects in addition to household composition variables (the number of additional pensions, an indicator for male presence, and the size of the household). Estimates reflect the treatment effect in the indicated year, relative to the baseline difference in 1981, the first available time period of data. Standard errors here are bootstrapped using the “wild bootstrap” technique outlined in Cameron et al. (2008).

Figure 3.11: Difference-in-Difference Year Coefficient Estimates for Future Eligible Females (Ages 35-44 in 1991)



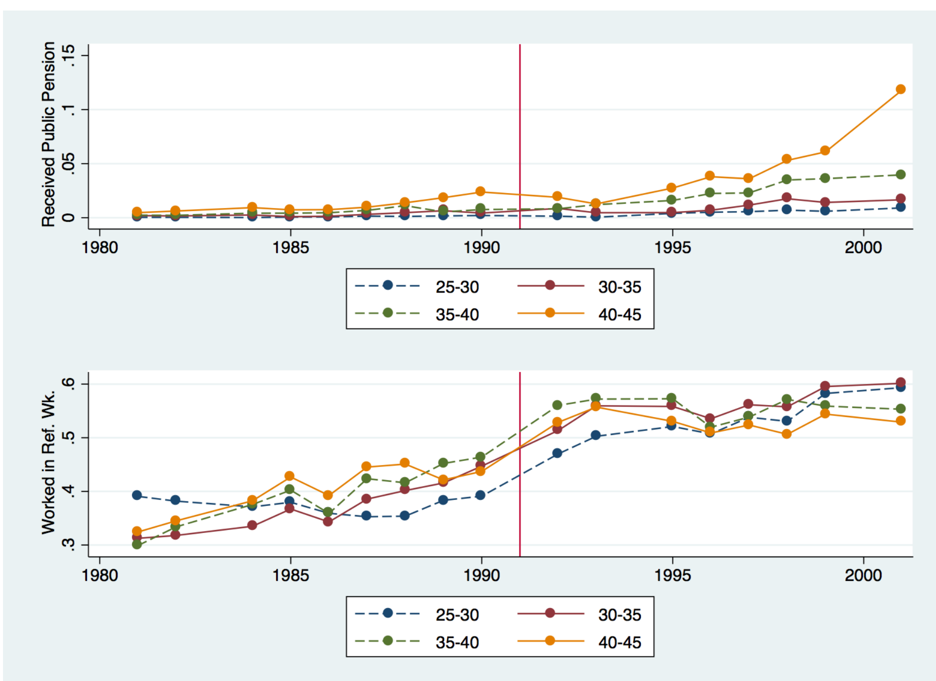
Note: Estimates are shown controlling for state and year fixed effects in addition to household composition variables (the number of additional pensions, an indicator for male presence, and the size of the household). Estimates reflect the treatment effect in the indicated year, relative to the baseline difference in 1981, the first available time period of data.

Figure 3.12: Difference-in-Difference Year Coefficient Estimates for Future Eligible Females (Ages 30-34 in 1991)



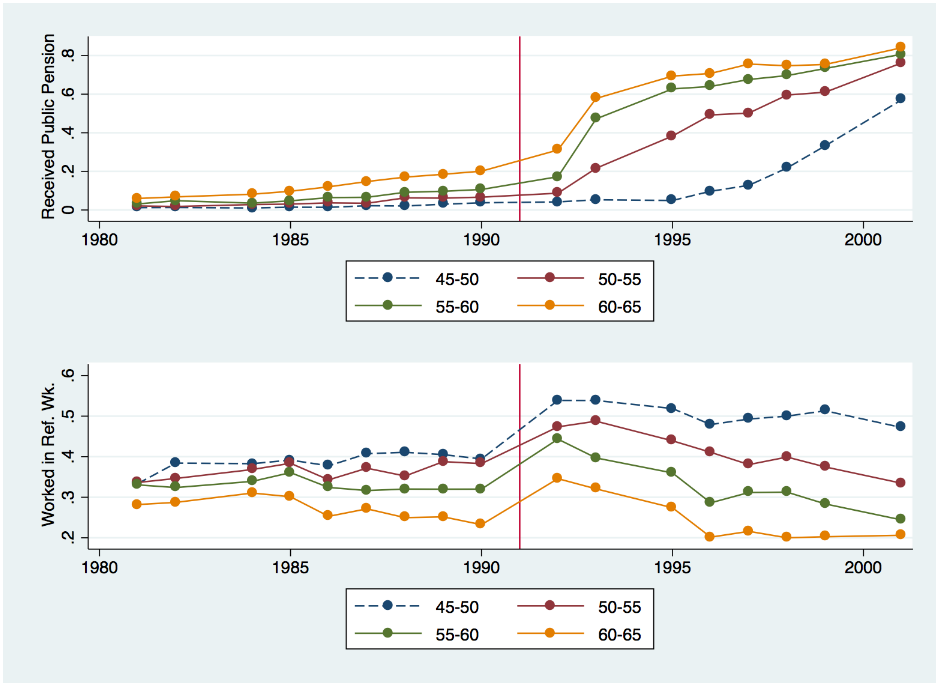
Note: Estimates are shown controlling for state and year fixed effects in addition to household composition variables (the number of additional pensions, an indicator for male presence, and the size of the household). Estimates reflect the treatment effect in the indicated year, relative to the baseline difference in 1981, the first available time period of data.

Figure 3.13: Outcomes for Young 1991 Rural Cohorts, by Age in 1991



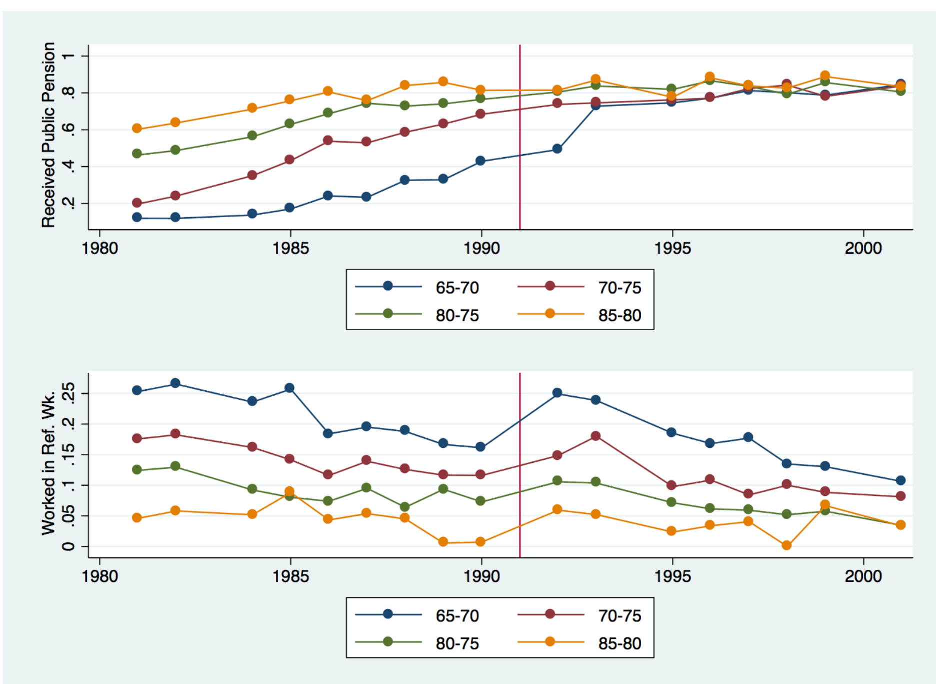
Note: Points represent unconditional age-group means.

Figure 3.14: Outcomes for Mid-Aged 1991 Rural Cohorts, by Age in 1991



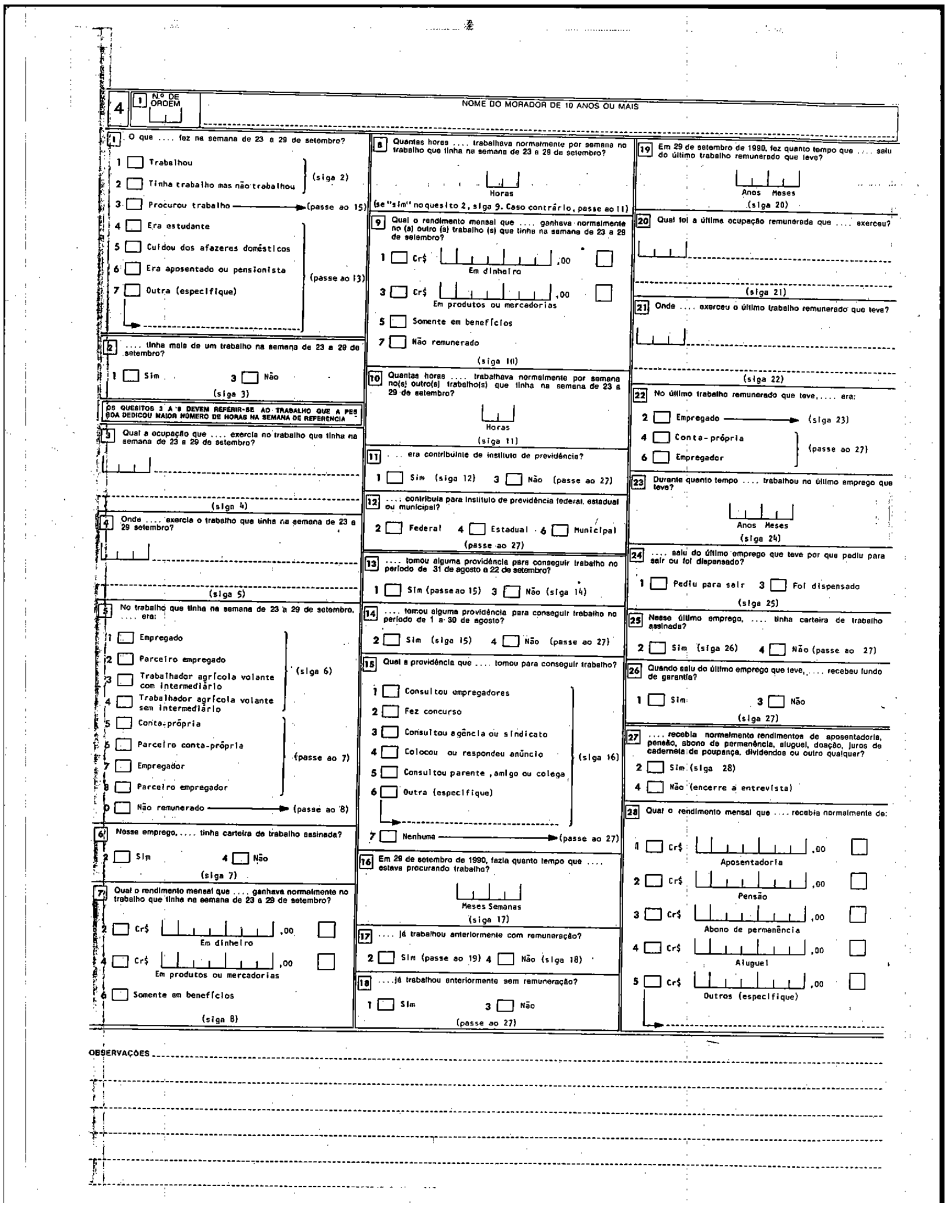
Note: Points represent unconditional age-group means.

Figure 3.15: Outcomes for Elderly 1991 Rural Cohorts, by Age in 1991



Note: Points represent unconditional age-group means.

Figure 3.16: PNAD Questionnaire (1990)



4 1 N.º DE ORDEM

NOME DO MORADOR DE 10 ANOS OU MAIS

1 O que .... fez na semana de 23 a 29 de setembro?
1 Trabalhou
2 Tinha trabalho mas não trabalhou (siga 2)
3 Procurou trabalho (passe ao 15)
4 Era estudante
5 Cuidou dos afazeres domésticos
6 Era aposentado ou pensionista
7 Outra (especifique) (passe ao 13)

2 .... tinha mais de um trabalho na semana de 23 a 29 de setembro?
1 Sim 3 Não
(siga 3)

OS QUESITOS 3 A 8 DEVEM REFERIR-SE AO TRABALHO QUE A PESSOA DEDICOU MAIOR NÚMERO DE HORAS NA SEMANA DE REFERÊNCIA

3 Qual a ocupação que .... exercia no trabalho que tinha na semana de 23 a 29 de setembro?
(siga 4)

4 Onde .... exercia o trabalho que tinha na semana de 23 a 29 setembro?
(siga 5)

5 No trabalho que tinha na semana de 23 a 29 de setembro, ... era:
1 Empregado
2 Parceiro empregado
3 Trabalhador agrícola volante com intermediário
4 Trabalhador agrícola volante sem intermediário (siga 6)
5 Conta-própria
6 Parceiro conta-própria
7 Empregador
8 Parceiro empregador (passe ao 7)
0 Não remunerado (passe ao 8)

6 Nesse emprego, ... tinha carteira de trabalho assinada?
2 Sim 4 Não
(siga 7)

7 Qual o rendimento mensal que .... ganhava normalmente no trabalho que tinha na semana de 23 a 29 de setembro?
2 Cr$,00 Em dinheiro
4 Cr$,00 Em produtos ou mercadorias
6 Somente em benefícios
(siga 8)

8 Quantas horas .... trabalhava normalmente por semana no trabalho que tinha na semana de 23 a 29 de setembro?
Horas
(se "sim" no quesito 2, siga 9. Caso contrário, passe ao 11)

9 Qual o rendimento mensal que .... ganhava normalmente no (s) outro (s) trabalho (s) que tinha na semana de 23 a 29 de setembro?
1 Cr$,00 Em dinheiro
3 Cr$,00 Em produtos ou mercadorias
5 Somente em benefícios
7 Não remunerado
(siga 10)

10 Quantas horas .... trabalhava normalmente por semana no(s) outro(s) trabalho(s) que tinha na semana de 23 a 29 de setembro?
Horas
(siga 11)

11 .... era contribuinte de instituto de previdência?
1 Sim (siga 12) 3 Não (passe ao 27)

12 .... contribuía para instituto de previdência federal, estadual ou municipal?
2 Federal 4 Estadual 6 Municipal
(passe ao 27)

13 .... tomou alguma providência para conseguir trabalho no período de 31 de agosto a 22 de setembro?
1 Sim (passe ao 15) 3 Não (siga 14)

14 .... tomou alguma providência para conseguir trabalho no período de 1 a 30 de agosto?
2 Sim (siga 15) 4 Não (passe ao 27)

15 Qual a providência que .... tomou para conseguir trabalho?
1 Consultou empregadores
2 Fez concurso
3 Consultou agência ou sindicato
4 Colocou ou respondeu anúncio
5 Consultou parente, amigo ou colega
6 Outra (especifique) (siga 16)
7 Nenhuma (passe ao 27)

16 Em 29 de setembro de 1990, fazia quanto tempo que .... estava procurando trabalho?
Meses Semanas
(siga 17)

17 .... já trabalhou anteriormente com remuneração?
2 Sim (passe ao 19) 4 Não (siga 18)

18 .... já trabalhou anteriormente sem remuneração?
1 Sim 3 Não
(passe ao 27)

19 Em 29 de setembro de 1990, fez quanto tempo que .... saiu do último trabalho remunerado que teve?
Anos Meses
(siga 20)

20 Qual foi a última ocupação remunerada que .... exerceu?
(siga 21)

21 Onde .... exerceu o último trabalho remunerado que teve?
(siga 22)

22 No último trabalho remunerado que teve, ... era:
2 Empregado (siga 23)
4 Conta-própria
6 Empregador (passe ao 27)

23 Durante quanto tempo .... trabalhou no último emprego que teve?
Anos Meses
(siga 24)

24 .... saiu do último emprego que teve por que pediu para sair ou foi dispensado?
1 Pediu para sair 3 Foi dispensado
(siga 25)

25 Nesse último emprego, ... tinha carteira de trabalho assinada?
2 Sim (siga 26) 4 Não (passe ao 27)

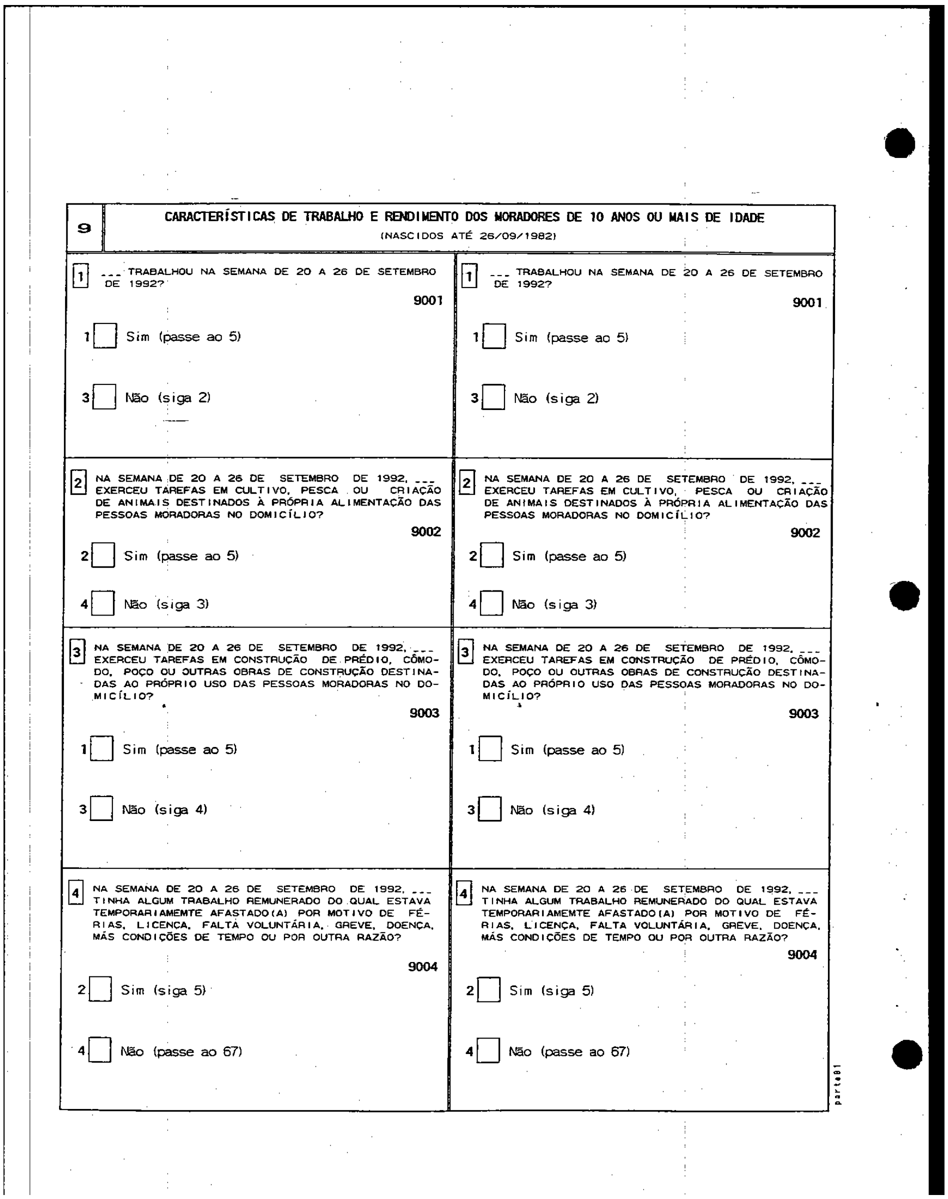
26 Quando saiu do último emprego que teve, ... recebeu fundo de garantia?
1 Sim 3 Não
(siga 27)

27 .... recebia normalmente rendimentos de aposentadoria, pensão, abono de permanência, aluguel, doação, juros de caderneta de poupança, dividendos ou outro qualquer?
2 Sim (siga 28)
4 Não (encerre a entrevista)

28 Qual o rendimento mensal que .... recebia normalmente de:
1 Cr$,00 Aposentadoria
2 Cr$,00 Pensão
3 Cr$,00 Abono de permanência
4 Cr$,00 Aluguel
5 Cr$,00 Outros (especifique)

OBSERVAÇÕES

Figure 3.17: PNAD Questionnaire (1992)



9 CARACTERÍSTICAS DE TRABALHO E RENDIMENTO DOS MORADORES DE 10 ANOS OU MAIS DE IDADE

(NASCIDOS ATÉ 26/09/1982)

1 ___ TRABALHOU NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992?

9001

1 Sim (passe ao 5)

3 Não (siga 2)

2 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ EXERCEU TAREFAS EM CULTIVO, PESCA OU CRIAÇÃO DE ANIMAIS DESTINADOS À PRÓPRIA ALIMENTAÇÃO DAS PESSOAS MORADORAS NO DOMICÍLIO?

9002

2 Sim (passe ao 5)

4 Não (siga 3)

3 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ EXERCEU TAREFAS EM CONSTRUÇÃO DE PRÉDIO, CÔMODO, POÇO OU OUTRAS OBRAS DE CONSTRUÇÃO DESTINADAS AO PRÓPRIO USO DAS PESSOAS MORADORAS NO DOMICÍLIO?

9003

1 Sim (passe ao 5)

3 Não (siga 4)

4 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ TINHA ALGUM TRABALHO REMUNERADO DO QUAL ESTAVA TEMPORARIAMEMTE AFASTADO(A) POR MOTIVO DE FÉRIAS, LICENÇA, FALTA VOLUNTÁRIA, GREVE, DOENÇA, MÁS CONDIÇÕES DE TEMPO OU POR OUTRA RAZÃO?

9004

2 Sim (siga 5)

4 Não (passe ao 67)

1 ___ TRABALHOU NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992?

9001

1 Sim (passe ao 5)

3 Não (siga 2)

2 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ EXERCEU TAREFAS EM CULTIVO, PESCA OU CRIAÇÃO DE ANIMAIS DESTINADOS À PRÓPRIA ALIMENTAÇÃO DAS PESSOAS MORADORAS NO DOMICÍLIO?

9002

2 Sim (passe ao 5)

4 Não (siga 3)

3 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ EXERCEU TAREFAS EM CONSTRUÇÃO DE PRÉDIO, CÔMODO, POÇO OU OUTRAS OBRAS DE CONSTRUÇÃO DESTINADAS AO PRÓPRIO USO DAS PESSOAS MORADORAS NO DOMICÍLIO?

9003

1 Sim (passe ao 5)

3 Não (siga 4)

4 NA SEMANA DE 20 A 26 DE SETEMBRO DE 1992, ___ TINHA ALGUM TRABALHO REMUNERADO DO QUAL ESTAVA TEMPORARIAMEMTE AFASTADO(A) POR MOTIVO DE FÉRIAS, LICENÇA, FALTA VOLUNTÁRIA, GREVE, DOENÇA, MÁS CONDIÇÕES DE TEMPO OU POR OUTRA RAZÃO?

9004

2 Sim (siga 5)

4 Não (passe ao 67)

parte01

3.9 Tables

Table 3.1: Transition Period for Working Activity Times in Lei 8213

Year	Contribution Time (Months)	Contribution Time (Years)
1991	60 months	5 years
1992	60 months	5 years
1993	66 months	5.5 years
1994	72 months	6 years
1995	78 months	6.5 years
1996	90 months	7.5 years
1997	96 months	8 years
1998	102 months	8.5 years
1999	108 months	9 years
2000	114 months	9.5 years
2001	120 months	10 years
2002	126 months	10.5 years
2003	132 months	11 years

Table 3.2: Main Sample Statistics: All Women

	mean	sd
Urban	0.82	0.38
Gov't. Pension Received	0.08	0.27
Worked	0.41	0.49
Working in Agriculture	0.06	0.23
Male Present in Household	0.92	0.28
Age	36.58	19.03
Lives by self	0.03	0.16
Number living in same HH	4.87	2.42
Classified as Spouse	0.54	0.50
Classified as Head	0.14	0.35
Observations	2132108	

Table 3.3: Main Sample Statistics: Rural Women

	mean	sd
Urban	0.00	0.00
Gov't. Pension Received	0.09	0.28
Worked	0.39	0.49
Working in Agriculture	0.26	0.44
Male Present in Household	0.96	0.20
Age	36.66	19.10
Lives by self	0.02	0.13
Number living in same HH	5.31	2.60
Classified as Spouse	0.62	0.48
Classified as Head	0.09	0.28
Observations	374164	

Table 3.4: Difference-in-Difference Year Coefficient Estimates for Urban/Rural Women

VARIABLES	(1) Received Pension	(2) Received Pension	(3) Worked	(4) Worked
Rural*1987	-0.000271	0.000627	-0.0164*	-0.0154
	(0.00273)	(0.00235)	(0.00948)	(0.00936)
Rural*1988	0.00305	0.00446	-0.0183	-0.0172
	(0.00307)	(0.00321)	(0.0158)	(0.0158)
Rural*1989	0.00307	0.00295	-0.00909	-0.00734
	(0.00295)	(0.00301)	(0.0130)	(0.0130)
Rural*1990	0.00389	0.00496	-0.00786	-0.00609
	(0.00329)	(0.00347)	(0.0110)	(0.0111)
Rural*1992	0.0153***	0.0127***	0.0625***	0.0657***
	(0.00513)	(0.00463)	(0.0138)	(0.0139)
Rural*1993	0.0336***	0.0299***	0.0800***	0.0844***
	(0.00742)	(0.00715)	(0.0114)	(0.0114)
Rural*1995	0.0442***	0.0396***	0.0476***	0.0522***
	(0.00836)	(0.00761)	(0.0102)	(0.0102)
Rural*1996	0.0426***	0.0380***	0.0372***	0.0423***
	(0.00945)	(0.00888)	(0.0126)	(0.0124)
Rural*1997+	0.0439***	0.0390***	0.0461***	0.0511***
	(0.00887)	(0.00798)	(0.0146)	(0.0145)
Constant	0.0197***	0.216***	0.348***	0.388***
	(0.00168)	(0.00827)	(0.00346)	(0.00831)
Observations	2,132,108	2,132,108	2,132,108	2,132,108
R-squared	0.009	0.064	0.009	0.014
Cluster	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household Comp Vars	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Difference-in-Difference Year Coefficient Estimates for Immediately Eligible Cohort

VARIABLES	(1) Received Pension	(2) Received Pension	(3) Worked	(4) Worked
Rural*1987	-0.0117	-0.0137	0.0126	0.0118
	(0.0132)	(0.0129)	(0.0187)	(0.0187)
Rural*1988	-0.00892	-0.0103	0.0305	0.0307
	(0.0101)	(0.0102)	(0.0283)	(0.0282)
Rural*1989	-0.0276**	-0.0293**	0.0456**	0.0463**
	(0.0110)	(0.0111)	(0.0201)	(0.0203)
Rural*1990	-0.0416***	-0.0427***	0.0575***	0.0593***
	(0.0130)	(0.0134)	(0.0199)	(0.0200)
Rural*1992	0.00147	-0.00331	0.191***	0.194***
	(0.0234)	(0.0232)	(0.0175)	(0.0172)
Rural*1993	0.189***	0.183***	0.186***	0.194***
	(0.0440)	(0.0436)	(0.0198)	(0.0201)
Rural*1995	0.257***	0.248***	0.161***	0.168***
	(0.0448)	(0.0442)	(0.0239)	(0.0237)
Rural*1996	0.236***	0.227***	0.137***	0.145***
	(0.0433)	(0.0434)	(0.0249)	(0.0250)
Rural*1997+	0.274***	0.264***	0.155***	0.161***
	(0.0452)	(0.0439)	(0.0220)	(0.0223)
Constant	0.0315***	0.177***	0.299***	0.410***
	(0.00748)	(0.0110)	(0.00380)	(0.00911)
Observations	189,488	189,488	189,488	189,488
R-squared	0.207	0.220	0.037	0.046
Cluster	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household Comp Vars	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Difference-in-Difference Year Coefficient Estimates for Immediately Eligible vs. Formerly Eligible Rural Women

VARIABLES	(1) Worked-Agri	(2) Worked-Agri	(3) Worked-Agri	(4) Worked-Agri
NewElig*1987	-0.0327**	-0.0327**	-0.0327**	-0.0327**
	(0.0150)	(0.0150)	(0.0150)	(0.0150)
NewElig*1988	-0.0189	-0.0189	-0.0189	-0.0189
	(0.0242)	(0.0242)	(0.0242)	(0.0242)
NewElig*1989	-0.0223	-0.0223	-0.0223	-0.0223
	(0.0165)	(0.0165)	(0.0165)	(0.0165)
NewElig*1990	-0.0299**	-0.0299**	-0.0299**	-0.0299**
	(0.0124)	(0.0124)	(0.0124)	(0.0124)
NewElig*1992	0.0997***	0.0997***	0.0997***	0.0997***
	(0.0171)	(0.0171)	(0.0171)	(0.0171)
NewElig*1993	0.0872***	0.0872***	0.0872***	0.0872***
	(0.0201)	(0.0201)	(0.0201)	(0.0201)
NewElig*1995	0.0433**	0.0433**	0.0433**	0.0433**
	(0.0199)	(0.0199)	(0.0199)	(0.0199)
NewElig*1996	0.00690	0.00690	0.00690	0.00690
	(0.0221)	(0.0221)	(0.0221)	(0.0221)
NewElig*1997+	0.0234	0.0234	0.0234	0.0234
	(0.0175)	(0.0175)	(0.0175)	(0.0175)
Constant	0.00376	0.00376	0.00376	0.00376
	(0.00477)	(0.00477)	(0.00477)	(0.00477)
Observations	186,324	186,324	186,324	186,324
R-squared	0.143	0.143	0.143	0.143
Cluster	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household Comp Vars	No	Yes	No	Yes

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.7: Difference-in-Difference Year Coefficient Estimates for Future Eligible Women (Ages 35-44 in 1991)

	(1)	(2)	(3)	(4)
VARIABLES	Received Pension	Received Pension	Worked	Worked
FutElig*1987	-0.000866	-3.40e-05	0.0247	0.0301**
	(0.00146)	(0.00151)	(0.0147)	(0.0146)
FutElig*1988	0.00255*	0.00317**	0.0146	0.0188
	(0.00147)	(0.00156)	(0.0195)	(0.0192)
FutElig*1989	-0.00107	-0.000457	0.0225	0.0254
	(0.00219)	(0.00228)	(0.0171)	(0.0171)
FutElig*1990	0.00149	0.00223	0.0212	0.0263*
	(0.00272)	(0.00282)	(0.0158)	(0.0154)
FutElig*1992	0.000418	0.000765	0.108***	0.112***
	(0.00204)	(0.00200)	(0.0215)	(0.0206)
FutElig*1993	-0.00564**	-0.00569**	0.135***	0.139***
	(0.00226)	(0.00236)	(0.0186)	(0.0188)
FutElig*1995	-0.00932***	-0.00919***	0.1000***	0.103***
	(0.00285)	(0.00280)	(0.0141)	(0.0140)
FutElig*1996	-0.0156***	-0.0154***	0.0990***	0.102***
	(0.00327)	(0.00324)	(0.0176)	(0.0177)
FutElig*1997+	-0.0277***	-0.0278***	0.113***	0.116***
	(0.00383)	(0.00378)	(0.0223)	(0.0220)
Constant	-0.00641***	0.0225***	0.399***	0.673***
	(0.000955)	(0.00146)	(0.00464)	(0.0134)
Observations	399,246	399,246	399,246	399,246
R-squared	0.029	0.034	0.020	0.036
Cluster	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household Comp Vars	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Difference-in-Difference Year Coefficient Estimates for Future Eligible Women (Ages 30-34 in 1991)

VARIABLES	(1) Received Pension	(2) Received Pension	(3) Worked	(4) Worked
FutElig*1987	-0.000866	-3.40e-05	0.0247	0.0301**
	(0.00146)	(0.00151)	(0.0147)	(0.0146)
FutElig*1988	0.00255*	0.00317**	0.0146	0.0188
	(0.00147)	(0.00156)	(0.0195)	(0.0192)
FutElig*1989	-0.00107	-0.000457	0.0225	0.0254
	(0.00219)	(0.00228)	(0.0171)	(0.0171)
FutElig*1990	0.00149	0.00223	0.0212	0.0263*
	(0.00272)	(0.00282)	(0.0158)	(0.0154)
FutElig*1992	0.000418	0.000765	0.108***	0.112***
	(0.00204)	(0.00200)	(0.0215)	(0.0206)
FutElig*1993	-0.00564**	-0.00569**	0.135***	0.139***
	(0.00226)	(0.00236)	(0.0186)	(0.0188)
FutElig*1995	-0.00932***	-0.00919***	0.1000***	0.103***
	(0.00285)	(0.00280)	(0.0141)	(0.0140)
FutElig*1996	-0.0156***	-0.0154***	0.0990***	0.102***
	(0.00327)	(0.00324)	(0.0176)	(0.0177)
FutElig*1997+	-0.0277***	-0.0278***	0.113***	0.116***
	(0.00383)	(0.00378)	(0.0223)	(0.0220)
Constant	-0.00641***	0.0225***	0.399***	0.673***
	(0.000955)	(0.00146)	(0.00464)	(0.0134)
Observations	399,246	399,246	399,246	399,246
R-squared	0.029	0.034	0.020	0.036
Cluster	State	State	State	State
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Household Comp Vars	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Appendix

Proof of Theorem 1

[label=App:AppendixA]

Proof. Consider the conditions under which $N^* = \bar{N}$. If $N = \bar{N}$ is an optimal choice, because

$$U(N|N > \bar{N}) = \bar{N}u(c_1^R) + (N^{*R} - \bar{N})u(c_2^R) + (T - \bar{N})u(c_3^R) + \sum_{t=N^{*R}}^{T} \alpha_i + \mu t$$

and

$$U(N|N < \bar{N}) = N^{*L}u(c_1^L) + (\bar{N} - N^{*L})u(c_2^L) + (T - \bar{N})u(c_3^L) + \sum_{t=N^{*L}}^{T} \alpha_i + \mu t$$

and the payoff to picking $N^* = \bar{N}$ is

$$U(N = \bar{N}) = \bar{N}u(c_1^M) + (T - \bar{N})u(c_2^M) + \sum_{t=\bar{N}}^{T} \alpha_i + \mu t,$$

it follows that

$$U(\bar{N}) > U(N|N > \bar{N})$$

$$\Longleftrightarrow$$

$$\bar{N}u(c_1^M) + (T - \bar{N})u(c_2^M) + \sum_{t=\bar{N}}^{T} \alpha_i + \mu t > \bar{N}u(c_1^R) + (N^{*R} - \bar{N})u(c_2^R) + (T - \bar{N})u(c_3^R) + \sum_{t=N^{*R}}^{T} \alpha_i + \mu t$$

and also that

$$U(\bar{N}) > U(N|N < \bar{N})$$

$$\Longleftrightarrow$$

$$\bar{N}u(c_1^M) + (T - \bar{N})u(c_2^M) + \sum_{t=\bar{N}}^{T} \alpha_i + \mu t > N^{*L}u(c_1^L) + (\bar{N} - N^{*L})u(c_2^L) + (T - \bar{N})u(c_3^L) + \sum_{t=N^{*L}}^{T} \alpha_i + \mu t$$

These statements are also equivalent, respectively, to

$$(\bar{N} - N^{*L})(\alpha_i + \mu\bar{N}(0.5)) < N^{*L} \ln\left(\frac{c_1^M}{c_1^L}\right) + (\bar{N} - N^{*L}) \ln\left(\frac{c_1^M}{c_2^L}\right) + (T - \bar{N}) \ln\left(\frac{c_2^M}{c_3^L}\right)$$

and

$$(N^{*R} - \bar{N})(\alpha_i + \mu N^{*R}(0.5)) > \bar{N} \ln\left(\frac{c_1^R}{c_1^M}\right) + (N^{*R} - \bar{N}) \ln\left(\frac{c_2^R}{c_1^M}\right) + (T - N^R) \ln\left(\frac{c_3^R}{c_2^M}\right)$$

Intuitively, in order for the choice of $\bar{N}$ to be optimal, the value of leisure must be high enough to compensate the worker for the increase in lifetime consumption he will gain from working for additional periods, however, it must not be so high as to prevent him from picking $\bar{N}$ over retiring earlier.

These two inequalities result in the bound

$$UB \equiv \left(N^{*L} \ln\left(\frac{c_1^M}{c_1^L}\right) + (\bar{N} - N^{*L}) \ln\left(\frac{c_1^M}{c_2^L}\right) + (T - \bar{N}) \ln\left(\frac{c_2^M}{c_3^L}\right)\right) \left(\frac{1}{(\bar{N} - N^{*L})}\right) - \mu\bar{N}(0.5)$$

$$> \alpha_i >$$

$$\left(\bar{N} \ln\left(\frac{c_1^R}{c_1^M}\right) + (N^{*R} - \bar{N}) \ln\left(\frac{c_2^R}{c_1^M}\right) + (T - N^R) \ln\left(\frac{c_3^R}{c_2^M}\right)\right) \left(\frac{1}{N^{*R} - \bar{N}}\right) - \mu N^{*R}(0.5) \equiv LB$$

The task now is to show that $\frac{\partial UB}{\partial d} >= 0$ and $\frac{\partial LB}{\partial d} < 0$. First note that by consumption smoothing, c_1^M and c_2^M depend completely on parameters. Specifically,

$$c_1^M = c_2^M = \frac{\bar{N}y + (T - \bar{N})d}{T} \text{ if } d < y$$

and

$$c_1^M = y \text{ and } c_2^M = d \text{ if } d > y$$

Intuitively, if the pension is outweighed by the wage, then workers are able and willing to completely smooth consumption by consuming a weighed average of the pension and the wage, with weights corresponding to the fraction of their lives spent "earning" each. However, if the pension outweighs the wage, then a worker is unable to smooth, however he comes as close as possible by consuming the most he can prior to eligibility – his wage.

Case 1 (d<y): If $d < y$, then $c_1^L = c_2^L = c_3^L = c^L$ and $c_1^M = c_2^M = c^M$. Furthermore, $c^L = \frac{N^{*L}y + d(T - \bar{N})}{T}$ and $c^M = \frac{\bar{N}y + d(T - \bar{N})}{T}$ by virtue of the budget constraint. Now, consider that N^{*L} satisfies the equality

$$N^{*L} = \frac{1}{\mu}\left(\frac{yT}{N^{*L}y + d(T - \bar{N})} - \alpha_i\right)$$

It follows that $\frac{\partial N^{*L}}{\partial d} < 0$ [13].

Now, note that $\frac{\partial c^M}{\partial d} = \frac{T - \bar{N}}{T}$ and that $0 < \frac{dc^L}{dd} < \frac{T - \bar{N}}{T}$. Therefore, it follows that $\frac{d \ln\left(\frac{c^M}{c^L}\right)}{dd} > 0$. Because

[13]To see this, consider that equation can be reorganized to form an equation in which the left side of the equation is increasing in both d and N^{*L}, and the right side is constant.

UB is an increasing function of both N^{*L} and $\ln\left(\frac{c^M}{c^L}\right)$, it follows that $\frac{\delta dUB}{\delta d} > 0$ [14].

Now, consider that $c^R = \frac{\bar{N}y+(N^{*R}-\bar{N})(y+d)+(T-N^{*R})d}{T}$ by virtue of the budget constraint, and also that N^{*R} satisfies the equality

$$N^{*R} = \frac{1}{\mu}\left(\frac{yT}{\bar{N}y+(N^{*R}-\bar{N})(y+d)+(T-N^{*R})d} - \alpha_i\right)$$

It follows that $\frac{\partial N^{*R}}{\partial d} < 0$ as above.

Further, note that $\frac{\partial c^M}{\partial d} = \frac{T-\bar{N}}{T}$ as above, and that $0 < \frac{dc^R}{dd} < \frac{T-\bar{N}}{T} = \frac{\partial c^M}{\partial d}$, so that $\frac{d\ln\left(\frac{c^R}{c^M}\right)}{dd} < 0$ – the denominator grows faster than the numerator as d increases, whereas before the numerator grew faster than the denominator. Now, LB is a *decreasing* function of N^{*R} *and* $\ln\left(\frac{c^R}{c^M}\right)$, so it follows that as d increases, LB decreases, expanding the bounds.

Case 2 (d>y): In the case of a larger pension value, the worker will be willing, but not able to consume the same amount in every period. In this sense, he reaches a "corner" solution as his consumption either pre-eligibility or post-eligibility is determined by either the wage or the pension amount. If he chooses to retire before eligibility, it follows that $c_1^L = c_2^L = c_{pre}^L$ and $c_3^L = d$. If he chooses to retire after eligibility, it follows that $c_1^R = y$ and $c_2^R = c_3^R = c_{post}^R$. However, the first order condition with respect to N does not change and instead becomes

$$\mu N^{*R} + \alpha_i = \frac{y}{c_{post}^R} \text{ if } N = N^{*R}$$

and

$$\mu N^{*L} + \alpha_i = \frac{y}{c_{pre}^R} \text{ if } N = N^{*L}$$

Because $c_{post}^R = \frac{(N^{*R}-\bar{N})y+(T-\bar{N})d}{T-\bar{N}}$ and $c_{pre}^L = \frac{N^{*L}y}{\bar{N}}$ these conditions then become

$$\mu N^{*R} + \alpha_i = \frac{y(T-\bar{N})}{(N^{*R}-\bar{N})y+(T-\bar{N})d} \text{ if } N = N^{*R}$$

and

$$\mu N^{*L} + \alpha_i = \frac{y\bar{N}}{N^{*L}} \text{ if } N = N^{*L}$$

Now, it follows that $\frac{\partial N^{*R}}{\partial d} < 0$ and that $\frac{\partial N^{*L}}{\partial d} = 0$. Additionally, by definition of c_{post}^R and c_{pre}^L, $\frac{dc_{post}^R}{dd} > 0$ and $\frac{dc_{pre}^L}{dd} = 0$. Hence, when d increases, the upper bound UB, being an increasing function of both c_{pre}^L and N^{*L}, does not change, where as the lower bound LB, being an increasing function of both c_{post}^R and

[14] To see this, consider that UB can be simplified to $T\ln\left(\frac{c^M}{c^L}\right)\left(\frac{1}{\bar{N}-N^{*L}}\right) - \mu\bar{N}(0.5)$, as the same amount is consumed in every period.

N^{*R}, decreases. Therefore, in all cases, when d increases, the distance $UB - LB$ increases.

■

All Eight Cutoffs

Throughout the paper we estimate the effect of government spending on labor market outcomes for municipalities around the first three FPM cutoffs. The first three cutoffs present the most significant discontinuity in terms of spending, as smaller municipalites are more dependent of FPM transfers (Litschig and Morrison 2013). Nonetheless, as a robustness check, in this section, we explore the discontinuity of on FPM transfers around all eight FPM cuttoffs. Using the same variable definitions from section 2.3, we first estimate the a reduced form model:

$$Y_{it} = \beta \sum_{j=1}^{8} 1[pop_{it} \geq c_j] \times seg_{itj} + \sum_{j=1}^{8} g_j(pop_{it} - c_j) \times seg_{itj} + \sum_{j=1}^{8} \alpha_j seg_{itj} + \delta_t + \mu_s + \theta X_i + \varepsilon_{it}$$

The results of these estimations as well as the estimation using the first three cutoffs, are reported on Table 2.16. Using the reduced form model, we estimate an slightly smaller effect of the eight FPM discontinuity on government spending than the first there cutoffs. This result likely reflects that bigger municipalities are less dependent on FPM transfers than bigger municipalities. Notably, Litschig and Morrison (2013) and Corbi et al. (2014) also find bigger discontinuity of government spending around the first FPM cutoffs, the first of which led Litschig and Morrison (2013) to restrict their analysis to municipalities around the first three cutoffs. In addition, there is a greater effect of the FPM discontinuity on the number of jobs when using the eight cutoffs rather than the three FPM cutoffs, although the effects are less precisely measured.

These reduced form estimations translate to the instrumental variable presented in Table 2.17. Using all the eight FPM cutoffs, we estimate that a 1% increase in government spending, roughly \$95,673 in the sample of municipalities around the first eight FPM cutoffs, is associated with the creation on 32.9 formal jobs. This estimate can be translated to a cost-per-formal-job-created of around \$2,908 per year, reflecting an even higher job multiplier effect of government spending in this expanded sample, although the effect is less precisely measured in the extended sample.

BIBLIOGRAPHY

A'Hearn, B., J. Baten, and D. Crayen (2009). Quantifying quantitative literacy: Age heaping and the history of human capital. *The Journal of Economic History 69*(3), 783–808.

Arbache, J. S. (2001). Wage differentials in Brazil: theory and evidence. *Journal of Development Studies 38*(2), 109–130.

Autor, D. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics 21*(1), 1–42.

Belloni, M. (2008). The option value model in the retirement literature: the trade-off between computation complexity and predictive validity. Research Report 50, ENEPRI.

Braga, B., S. Firpo, and G. Gonzaga (2009). Escolaridade e o diferencial de rendimentos entre o setor privado e o setor público no Brasil. *Pesquisa e Planejamento Econômico 39*(3), 431–464.

Brollo, F., T. Nannicini, R. Perotti, and G. Tabellini (2013). The political resource curse. *The American Economic Review 103*(5), 1759–1796.

Calonico, S., M. Cattaneo, and R. Titiunik (2014a). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014b). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Caselli, F. and G. Michaels (2013). Do oil windfalls improve living standards? Evidence from Brazil. *American Economic Journal: Applied Economics 5*(1), 208–38.

Chodorow-Reich, G., L. Feiveson, Z. Liscow, and W. G. Woolston (2012). Does state fiscal relief during recessions increase employment? Evidence from the American Recovery and Reinvestment Act. *American Economic Journal: Economic Policy*, 118–145.

Coile, C. and J. Gruber (2007). Future social security entitlements and the retirement decision. *Review of Economics and Statistics 89*(2), 234–246.

Corbi, R., E. Papaioannou, and P. Surico (2014). Federal transfer multipliers. Quasi-experimental evidence from Brazil. Technical report, National Bureau of Economic Research.

da Silva, L. I. L. (2012, February). The brazilian social security system - a peaceful revolution. Online editorial.

David, H. and D. Dorn (2013). The growth of low-skill service jobs and the polarization of the US labor market. *The American Economic Review 103*(5), 1553–1597.

de Carvalho Filho, I. E. (2008). Old-age benefits and retirement decisions of rural elderly in brazil. *Journal of Development Economics 86*, 129–146.

de Carvalho Filho, I. E. (2012). Household income as a determinant of child labor and school enrollment in brazil: Evidence fro a social security reform. *Economic Development and Cultural Change 60*(2), 399–435.

French, E. (2005). The effects of health, wealth, and wages on labour supply and retirement behavior. *The Review of Economic Studies 72*(2), 395–427.

Friedman, M. (1957). *A Theory of the Consumption Function*. Princeton University Press.

Gruber, J. and D. A. Wise (Eds.) (1999). *Social Security and Retirement and the World* (1st ed.). University of Chicago.

Gustman, A. and T. Steinmeier (1994). Employer-provided health insurance and retirement behavior. *Industrial and Labor Relations Review 48*(1), 124–140.

Hahn, J., P. Todd, and W. van der Klaauw (2001). Identification and estimation of treatment effects with a regression discontinuity design. *Econometrica 69*(1), 201–209.

Imbens, G. W. and T. Lemieux (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics 142*(2), 615–635.

Imbert, C. and J. Papp (2012). Labor market effects of social programs: Evidence from india's employment guarantee. *American Economic Journal: Applied Economics 7*(2), 233–263.

Kahn, J. A. (1988). Social security, liquidity, and early retirement. *Journal of Public Economics 35*(1), 97–117.

Kraay, A. (2012). How large is the government spending multiplier? Evidence from World Bank lending. *Quarterly Journal of Economics 127*(2).

Kraay, A. (2014). Government spending multipliers in developing countries: evidence from lending by official creditors. *American Economic Journal: Macroeconomics 6*(4), 170–208.

Lee, D. S. and T. Lemieux (2010a). Regression discontinuity designs in economics. *Journal of Economic Literature 48*(2), 281–355.

Lee, D. S. and T. Lemieux (2010b). Regression discontinuity designs in economics. *Journal of Economic Literature*, 281–355.

Legrand, T. K. (1995). The determinants of men's retirement behaviour in brazil. *The Journal of Development Studies 31*(5), 673–701.

Litschig, S. (2012). Are rules-based government programs shielded from special-interest politics? evidence from revenue-sharing transfers in brazil? *Journal of Public Economics 96*, 1047–1060.

Litschig, S. and K. M. Morrison (2013). The impact of intergovernmental transfers on education outcomes and poverty reduction. *American Economic Journal: Applied Economics 5*(4), 206–240.

Macurdy, T. E. (1981). An empirical model of labor supply in a life cycle setting. *Journal of Political Economy 89*(6), 1059–1085.

McClelland, R. and S. Mok (2012). A review of recent resarch on labor supply elasticities. *Working Paper Series, Congressional Budget Office*.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics 142*(2), 698–714.

Monasterio, L. (2013). O FPM e a estranha distribuição da população dos pequenos municípios brasileiros. *unpublished paper,*.

Neumark, D. and E. Powers (1998, May). The effect of means-tested income support for the elderly on pre-retirement saving: evidence from the ssi program in the u.s. *Journal of Public Economics 68*(2), 181–206.

Neumark, D. and E. Powers (2000, October). Welfare for the elderly: the effects of ssi on pre-retirement labor supply. *Journal of Public Economics 78*(1-2), 51–80.

Neumark, D. and E. Powers (2004). The effect of the ssi program on labor supply: Improved evidence from social security administrative files. *Social Security Bulletin 65*(3), 45–60.

Ponczek, V. (2011). Income and bargaining effects on education and health in brazil. *Journal of Development Economics 94*, 242–253.

Powers, E. T. (1998, April). Does means-testing welfare discourage saving? evidence from a change in afdc policy in the united states. *Journal of Public Economics 68*(1), 33–53.

Queiroz, B. L. and M. G. B. Figoli (2010). The social protection system for the elderly in brazil. Working Paper.

Ranchhod, V. (2006). The effect of the south african old age pension on labour supply of the elderly. *South African Journal of Economics 74*(4), 725–744.

Rust, J. and C. Phelan (1997). How social security and medicare affect retirement in a world with incomplete markets. *Econometrica 65*(4), 781–831.

Serrato, J. C. S. and P. Wingender (2014). Estimating the incidence of government spending. *unpublished paper,*.

Shoag, D. (2010). The impact of government spending shocks: Evidence on the multiplier from state pension plan returns. *unpublished paper, Harvard University*.

Soares, R. R. (2010). Aging, retirement, and labor market in brazil. Unpublished Manuscript.

SSA (2015a, September). International update. Bulletin.

SSA (2015b, May). International update. Bulletin.

SSA (2016, February). International update. Bulletin.

Stock, J. H. and D. A. Wise (1990). Pensions, the option value of work, and retirement. *Econometrica 58*(5), 1151–1180.

Ulyssea, G. (2014). Firms, informality and development: Theory and evidence from Brazil. Technical report, Department of Economics PUC-Rio (Brazil).

Wilson, D. J. (2012). Fiscal spending jobs multipliers: Evidence from the 2009 American Recovery and Reinvestment Act. *American Economic Journal: Economic Policy*, 251–282.